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AND  
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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster  
vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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VOL. II.—FIFTH SERIES.  
JULY—DECEMBER 1876.

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LONDON.

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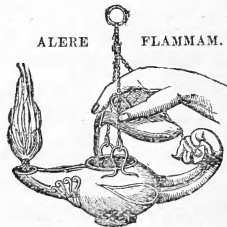
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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

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—“Cur spirent venti, cur terra dehiscat,  
Cur mare turgescat, pelago cur tantus amaror,  
Cur caput obscura Phœbus ferrugine condât,  
Quid toties diros cogat flagrare cometas;  
Quid pariat nubes, veniant cur fulmina cœlo,  
Quo micet igne Iris, superos quis conciat orbes  
Tam vario motu.”

*J. B. Pinelli ad Mazonium.*



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## ERRATA.

Vol. I., at the bottom of p. 547, for  $\frac{160}{156}$  read  $\frac{160}{156}$

$$\begin{array}{r} \frac{160}{156} \\ \hline \frac{40}{39} \\ \hline 1 \end{array} \quad \begin{array}{r} \frac{156}{156} \\ \hline \frac{40}{39} \\ \hline 1 \end{array}$$

„ page 549, line 5, for  $3 \cdot \frac{1}{U_{10}^{19}}$  read  $3 \cdot \frac{1}{U_{10}^{-19}}$

Vol. II., page 373, line 4, for  $xy$  read  $x=y$ .



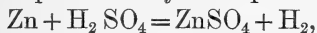
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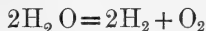
JULY 1876.

I. *On Chemical Notation.* By M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, the Owens College, Manchester\*.

1. **I**N examining a chemical action we may pay attention to (1) the substances which take part in, and the final products of, the action, or (2) to the force which is concerned in doing the work. Thus the action of sulphuric acid upon zinc is commonly represented by the equation



which tells us that a given weight of zinc acts upon, or is acted upon by, a given weight of sulphuric acid, with the production of certain fixed weights of zinc sulphate and of hydrogen; and further, the equation implies that a certain amount of force is expended in the action. So again the equation



not only states, in symbolic language, the fact that 36 parts by weight of water yield 4 parts by weight of hydrogen, and 32 parts by weight of oxygen, but it also implies the further fact that, in order to obtain the quantities of hydrogen and of oxygen from water, a certain amount of energy must be altered in form. The latter fact is not, it is true, stated in our ordinary chemical equations in the same prominent manner as the former; still it is, I think, unmistakably implied.

2. These two methods of regarding a chemical equation are not without close relationships to each other. In a paper

\* Communicated by the Author.

published in this Magazine by Dr. E. J. Mills, it is said that chemical substances are valued not for what they are conceived as being, but as doing—that the most important question with regard to a chemical substance is, how does it behave with this or that reagent? It appears to me that what a chemical substance *does* depends very much upon what it *is*, and that, although we are obliged to study these questions to a certain extent apart, we shall some day be able to express by one and the same formula the constitution and the various modes of action of all known chemical bodies.

3. In the following paper I shall endeavour to point out what appear to me to be some of the reasons for maintaining our ordinary formulæ, but at the same time, for supplementing them by investigations into, and symbolic representations of, the dynamical laws governing chemical transformations.

4. In the first place, all questions as to work done by different chemical substances imply the existence of these substances: change in the form of energy implies the existence of energy; but the existence of energy implies, to our minds at any rate, a something which is the seat or vehicle of energy. This something is matter. Matter has been defined by two eminent naturalists as “that which is essential to the existence of the known forms of energy, without which therefore there could be no transformations of energy.” The physical universe is often compared to a machine, the laws of the working of which are the laws of energy; but an investigation into these laws surely does not preclude, but rather prompts to an investigation into the structure of the machine itself. It may be urged that what we call matter has no existence outside of the mind of the observer; this objection, however, has been often refuted\*.

5. One of the most well-founded generalizations concerning matter is to the effect that no particle of it can be destroyed, nor be produced by us. The mass of matter cannot be changed by us: its form may be changed, the forms of its energy may be altered; but its quantity remains the same. If

\* ‘Unseen Universe,’ 1st edit. p. 71. “Some extreme thinkers write as if they would persuade us that a species of hallucination affects with similar impressions every individual mind, so that, for instance, one man may usefully warn another about a pitfall on a dark road, and so save him from a catastrophe which might otherwise be caused by something which exists, if at all, in the mentor’s mind only,—at all events not as yet in that of his pupil; though if the warning be unheeded, or not given, there will presently be another mind in which the pitfall will certainly exist with startling vividness. But this is altogether repugnant to every conviction which experience (our only guide in such matters) enables us to form, &c.”

this be so, surely it becomes of vital importance to every naturalist—and in the term naturalist we include the chemist—to have a symbolical language which shall express the weights of matter taking part in, or resulting from, any action between what we call different kinds of matter.

Now whatever theory we may hold concerning the constitution of matter, it can scarcely be denied that our ordinary formulæ do tell us the weights of those kinds of matter which take part in a given chemical reaction, or of those which result from that reaction. To the principle of the conservation of matter the writer already quoted appears to object. He would seemingly refuse his assent unless some one could experimentally prove to him that a given chemical compound has the same composition now that it had a hundred years ago.

Surely such a statement as this implies ignorance of the meanings of a scientific generalization. Elsewhere Dr. Mills speaks of the ideal reformer as one who “must have an infinite capability of doubting.” Is the infinity of this capability to cause the ideal reformer to doubt every generalization which rests upon well known and most thoroughly trustworthy facts, because the special proof which *he* demands cannot, and that from the very nature of the case, be forthcoming?

6. A chemical equation expresses the weights of the different kinds of matter taking part in the reaction formulated; it therefore implies a dissimilarity of some sort between the *kinds* of matter. The equation  $2\text{H}_2 + \text{O}_2 = 2\text{H}_2\text{O}$  implies that the kind of matter represented by the symbol  $2\text{H}_2$  is different in its essential properties from that represented by either of the symbols  $\text{O}_2$  or  $2\text{H}_2\text{O}$ ,—and, further, that the kind of matter indicated by the last-mentioned symbol is in some way composed of the two other kinds of matter. To such an expression it has been objected that we do not know that the substance represented by the symbol  $2\text{H}_2\text{O}$  is *composed* of the two substances  $2\text{H}_2$  and  $\text{O}_2$ , that we have reasons for believing that each of these three substances is equally homogeneous. Now it appears to me that we may use such a chemical equation without committing ourselves to any theory as to the manner in which the hydrogen and oxygen unite together to form water. When we say in chemical shorthand, 4 parts by weight of hydrogen, under certain conditions, so react with 32 parts by weight of oxygen as to produce 36 parts by weight of water, we state a fact. Here are three substances which exhibit to our senses different phenomena; with each of these substances certain definite phenomena are invariably associated; and we find that, under

fixed conditions, from a mixture of certain weights of the two former a weight of the third equal to the sum of these two weights is invariably produced. In such a statement nothing is said of the homogeneity or non-homogeneity of the three substances: whatever views may be held on this subject, the fact of the production of the third substance by the mutual action of the other two is undeniable; and this fact is symbolically expressed in the equation we have been considering.

Of course chemists are generally in the habit of conceiving that water is *made up of* oxygen and hydrogen, that these substances exist in some way in the new substance formed by their action upon one another; and the equation will bear this meaning. On the other hand, it is at least possible, in the light of Mr. Lockyer's recent spectral researches, to imagine that hydrogen and oxygen may themselves be split up into simpler substances (just as water is split up into hydrogen and oxygen); but if this be the case the equation may remain unaltered: it expresses a truth, although probably not the whole truth. But we believe that chemical equations tell us much more about the composition of substances than the mere fact that certain weights of two bodies produce a certain weight of a third; this part of the subject, however, I shall defer to a later portion of the paper.

7. If we are to regard chemical substances chiefly in the light of what they *do*, it may, apparently, be urged that our ordinary formulæ are of little use to us—that, inasmuch as these formulæ are based upon statical ideas, they are useless, or all but useless, when we come to deal with questions of kinetics.

If we investigate chemical formulæ, especially in their more recent outcome, I think we shall see that this is a mistaken idea.

8. Some chemists have affected to sneer at the “pictures,” as they call them, which “deface” the pages of so many modern treatises. If they have convinced themselves that “Ignorance is bliss,” then they are right in the conclusion “’Tis folly to be wise.” That the use of structural or dissected formulæ may be carried to a pitch which does become ridiculous—especially in the case of compounds about which we know little or nothing, but whose graphic formulæ are, it would appear for that reason, to be continually sketched—can scarcely be denied. But why do chemists make use of such formulæ at all? is it not because they wish to express in the shortest possible manner the greatest amount of information about the *actions* of the substances formulated? Instead of putting down in words what this body will do under certain



circumstances, how this body will react with other bodies, chemists seek to compress all they know of the *doings* of the substance into that shorthand expression by which they distinguish it from all other bodies. To say that structural formulæ do not tell us much about the actual, and even the possible, *actions* of the substances formulated is simply to confess to ignorance of what these formulæ exhibit.

9. For proof of this statement I might refer to many researches; let one suffice.

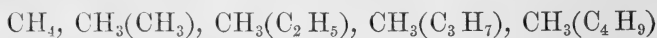
In a remarkable paper published in this Magazine\*, Dr. C. R. A. Wright examined the relations between affinity and dissected or structural formulæ in the case of certain groups of compounds; and he showed that the dissected formula of a chemical compound may give us much information, not only with regard to the method of formation, and decomposition under the influence of various reagents, of that compound, but also with regard to the transformations of energy which take place during these actions. Thus, to take an example, it is shown that when an operation, "symbolically represented by the substitution of the group of symbols  $\text{CH}_3$  for the symbol H in some constituent hydrocarbonous radical ( $\text{CH}_3$ ,  $\text{CH}_2$ , or CH) in the dissected formula of the original substance," is performed upon a given vapour, then "heat is evolved during the performance of the operation," and the new and more dense vapour produced has a higher "affinity-value" than the original vapour. Here we have a most important generalization with regard to homologous series of compounds: as the series is ascended the "affinity-value" increases; and these differences of affinity-values are "correlative with the differences between . . . dissected formulæ indicating substances chemically related together."

On the other hand, Dr. Wright shows that substitution of the group  $\text{CH}_3$  for H in the OH group in the dissected formula of a vapour is attended with absorption of heat, and therefore with decrease in affinity-value: thus the conversion of an acid into its methylic ether is attended with absorption of heat.

10. It may be urged that our formulæ do not express such relationships as those between transformations of energy and chemical action. At present they do not; but it would require only the addition of a symbolic expression of some kind, not the discarding of the formulæ and the invention of entirely new expressions, to enable us to express such relationships. Dr. Wright's researches show (and this is the main point to be insisted upon) that there *is* a connexion between the com-

\* December 1874.

monly employed dissected or structural formulæ and changes in the energy of the substances formulated. This conclusion might almost have been arrived at by a study of structural formulæ themselves. Such formulæ as



tell us that two volumes of each hydrocarbon vapour may be prepared from two volumes of the vapour of the hydrocarbon immediately preceding it by an operation symbolically represented by the replacement of H by a radical having the general formula  $\text{C}_n\text{H}_{2n+1}$ . Now there must be some change in the energy of these hydrocarbons before and after the performance of such operations; and inasmuch as we can ascend the series by a repetition of the same operation, we should expect that the change in energy, whatever it may be, would be regular, and would be related in some way to the regular change in chemical constitution.

11. Not only do the formulæ of compounds indicate the actions by which these substances are produced, and by which they may be transformed into other substances, but with the formulæ of the elementary bodies there is likewise associated information of a similar nature.

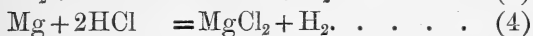
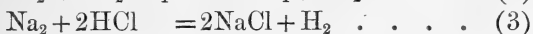
Mendelejeff\* has pointed out that if the symbols of the elements be arranged in the order of their combining weights, beginning with the smallest, the elements may be divided into groups and series, the members of which are characterized by a gradation of properties, which gradation is closely related to the increase of the combining number. Each group possesses many properties in common, among these the power of forming oxides and other salts of similar type being very marked. We have here, then, a possible arrangement of chemical symbols such that, from its position in a regular series, each symbol shall tell us a great deal concerning the *power of doing* of the element which it symbolizes.

12. It appears to me, therefore, that our chemical formulæ do tell us very much about the *actions* of the substances formulated, about their methods of formation and of decomposition, and that as advances are made in a knowledge of the changes of energy, which are undoubtedly correlated with changes in chemical constitution, these formulæ may be so modified as to express such knowledge. At present we know so little of chemical kinetics that it would be altogether premature to propose any method for giving symbolic expression to the few facts which have been brought to light.

13. In the symbolic expressions of chemical actions such

\* *Ann. Chem. Pharm.* Suppl. vol. viii. p. 133.

statements occur as these :—



By comparing equations (1) and (2), it is evident that the quantity of potassium expressed by the symbol K has replaced, in sulphuric acid, a quantity of hydrogen represented by the symbol H, while twice this quantity of hydrogen has been replaced by the symbol Ca. The conclusion, therefore, to be deduced from these equations is, that, so far as the power of replacing hydrogen in sulphuric acid is concerned, the symbol Ca is equivalent to the symbol K taken twice. Similarly from a comparison of (3) and (4) it may be concluded that Mg is equivalent, in hydrogen-replacing power, to 2Na. From a consideration of such equations as these the idea of valency takes its origin. This idea has been largely developed; and chemists have been able to fix the valency of a large number of radicles, whether simple or compound. The valency of a chemical substance expresses a certain function which that substance is capable of performing; it tells us that the substance under consideration is capable of doing a certain amount of work; but inasmuch as the circumstances under which this work is done vary, the valency also will vary with the circumstances. Nevertheless we find that for each elementary radicle there is a fixed maximum valency; we have therefore a most useful means of classifying these radicles according to their power of doing a certain work. It is true that the unit of work done is not very strictly defined; it is only the unit weight of hydrogen displaced, or combined with. Yet this classification is in practice found to be most useful. Given the valency of a metallic radicle, and the basicity of an acid upon which that radicle exercises an action, it is possible to foretell what the composition of the resultant of that action will be. Hence those quantities of two or more radicles which are able to replace the same quantity of hydrogen may be truly said to be "equivalent" quantities.

14. Dr. Mills (*loc. cit.*) objects to this use of the word equivalent, and avers that "we might as well say that the values of the loads in a railway truck are always equivalent, whether the load consists of air, of hay, or bullion." They are equivalent in one respect, viz. in that they have displaced equal volumes of air. We have nothing to do with the general *value* of a radicle when we speak of its valency in the com-

monly accepted meaning of the phrase. That amount of a given radicle which will displace unit weight of hydrogen from an acid may not be that amount which will do some other kind of work, just as that amount of hay which will displace 1 cubic foot of air in the railway truck is *not* that amount which will do the work of an equal volume of bullion in so far as exchange is concerned, although it does accomplish the same amount of work when we define the unit of work as "replacing 1 cubic foot of air." Dr. Mills has introduced an unfortunate word by using the term *value*.

15. If we for a moment agree to view this matter of valency in the light of the molecular theory of matter, and of some recent researches of Michaelis and other chemists, the explanation given of the relation between valency and general chemical "value" would be something as follows. We may imagine the molecule of a chemical element performing a certain vibration in a definite period of time; during the performance of this vibration there are certain positions attained by the molecule, at each of which it is so situated as to be capable of exercising chemical action upon other molecules. The number of these positions is the valency of the molecule. But we can suppose that the total force exercised is not uniformly distributed, so that an unequal amount is capable of being exercised at each position; hence the valency may vary according to the reactions in which the molecule takes part. Again, it is clear that the total force exercised will not necessarily bear a simple relation to the number of positions in which the molecule may be capable of exercising this force; hence the general affinity (or chemical "value") of a molecule may be small, while its valency is large, or *vice versâ*.

16. Altogether, then, I think we have in valency a most useful means of classification, and that we are fully entitled to use the term "equivalent" in the acceptation generally assigned to it by chemists. This, however, does not prevent us from admiring Dr. Mills's proposal to determine the power of doing work, in terms of a fixed unit, of classes of chemical substances. Undoubtedly such investigations would be highly interesting, and would lead to most important results; but why we need do away with the advantages gained in order to gain further advantages I, for one, am at a loss to determine. Dr. Mills has himself made some very careful and elegant experiments, by which he has determined the "dynamic equivalent" of various nitrates in terms of a fixed unit. It would be well that these researches should be extended.

But when we are in the possession of a large amount of such information as this, surely it will not be necessary to

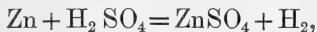
devise an entirely new system of notation, but only to modify the existing system so as to include the new knowledge. Our present notation is founded on facts; it does not necessarily involve any theory, only a generalization, as Dr. Wright has well pointed out; hence it is not to be thrown aside because at some future time it may not adequately express all our knowledge. Let us wait till that knowledge is gained, and then try to so modify our notation as to symbolize the knowledge.

But if we should not find it possible to do this, and it becomes necessary to have a new system of notation, it will be still a system which has been developed from, not a system which has overthrown, the old.

17. I have so far endeavoured to put forward reasons for believing that our existing system of notation symbolizes, to a certain extent, both the substances which take part in, and the final products of a chemical action, and also some of the relations existing between these substances and the changes of energy which accompany the action. In so far as the latter changes are concerned, it must, however, be confessed that our ignorance is great, and that when more knowledge is gained it will be necessary for us to modify our notation so as to express this knowledge symbolically.

I have sought to show that our equations represent the initial and final distributions of matter during a reaction symbolized, that they imply changes of energy, that in the case of dissected or structural formulæ we can trace a close connexion between such changes of energy and the formulæ, and that in valency we have at any rate the rudiments of a method by which the power of doing work of different substances may be compared.

18. On looking at such an equation as



one might imagine that the weight of zinc sulphate expressed by the symbols  $\text{ZnSO}_4$  would be at once produced on the addition of that quantity of sulphuric acid represented by  $\text{H}_2 \text{SO}_4$  to Zn parts by weight of zinc, and that, were a somewhat larger quantity of acid to be added to the same quantity of zinc, the action would not be modified. The equation appears to convey this meaning; but the meaning is erroneous. Chemical action involves greater or less quantities of time; and it is certainly influenced by the total mass of the acting substances, as also by the mass of the resultants, and by the relation which these bear at any moment to the generating substances.

19. Hitherto we have not been able to express such facts as

these symbolically. And at present it would be useless to attempt such a task; we are but on the threshold of such inquiries; we have been so long (perhaps too long) occupied with gathering together facts concerning the substances produced by chemical action that we have had little time to devote to a closer study of the action itself in its various phases. Such study will form a part of the chemistry that is to be, and will without doubt be rich in results of the highest importance. But granted that we cannot now express the process of a chemical change in our notation, this does not at all prove that the notation is founded on a false system. We know that one of the best-grounded generalizations of science is expressed by saying, that those substances which we call chemical elements, when they react together so as to form compounds (that is, bodies in which the characteristic properties of the reacting elements are merged), do so in simple multiples of a certain fixed number,—that the composition of the compounds may therefore be expressed by writing the names (or symbols) of the elements, with numbers attached to each expressive of the multiple of the certain fixed number which had before been assigned to each element\*.

This statement merely generalizes a vast number of observed facts. Unless we are to take the results of each individual experiment as expressive of the exact truth as regards the composition of the substance under examination, in which case a science of chemistry would be impossible, we must accept such a statement as this. As our chemical notation is founded on this generalization from facts as one of its main supports, it follows that the results of future experiments in the direction of tracing the exact steps of chemical action, and of measuring the various forces which there come into play, whatever the results may be, cannot overthrow our present system of notation; they may cause us to modify it greatly, perhaps so greatly that one who had been accustomed to the old would hardly recognize the new notation as in any way connected with it. Nevertheless we may be certain that the process will involve no breach of continuity.

20. But I must now pass on to consider chemical notation in the light of that theory which appears to me to explain the best-known facts of material science in a more thorough and satisfactory manner than any which has as yet been proposed: I mean the Molecular Theory of Matter.

21. And in the first place I would insist upon the truth of Dr. Wright's remark (*loc. cit.*), that neither this theory nor the atomic theory, generally so called, is taken for granted in

\* See Wright, *Phil. Mag.* [IV.] vol. xliii. p. 505.

the formation of chemical equations, or in the general methods of chemical research. The meaning of the words "atomic theory," and the distinction between the generalization and convention upon which our chemical notation is really based, and the hypothesis upon which it is so often *said* to be based, are so ably pointed out by Dr. Wright in the paper referred to\*, and in a previous paper, that I need do no more than refer any one who wishes for clear ideas on the subject to those papers.

22. Premising, then, that chemical notation does not assume the existence of "atoms," but admitting that if the atomic hypothesis were granted the existing chemical notation would readily adapt itself to the necessities of an atomic notation, let me pass on to consider some of the reasons which appear to me to favour the adoption of the molecular theory of matter. "A molecule may be defined as a small mass of matter, the parts of which do not part company during the excursions which the molecule makes when the body to which it belongs is hot." This quotation from Prof. Clerk Maxwell's 'Treatise on the Theory of Heat,' gives a clear definition of what is meant by the term molecule. The theory in question supposes that any given piece of matter consists of a definite number of molecules, that all the molecules of the same substance contain the same quantity of matter, that a molecule may consist of "several distinct portions of matter held together by chemical bonds," and, finally, that "the molecules of all bodies are in a state of continual agitation."

23. The molecular theory of matter is to be distinguished from a theory which would assert that there is an absolute limit to the divisibility of matter. What the theory asserts is, to use Prof. Maxwell's words, "that after we have divided a body into a certain finite number of constituent parts called molecules, then any further division of these molecules will deprive them of the properties which give rise to the phenomena observed in the substance."

24. The "Daltonian atomic theory" cannot of course be regarded as identical with that which I have just sketched. It appears to me, however, that the atomic theory of the chemist is based upon and is an outcome of the molecular theory of the physicist, and that we can only rightly understand the former when we have gained a knowledge of the latter,—in fact that the atomic theory, so called, is implied in the molecular theory of matter, and stands or falls with it.

25. One of the objections raised by Dr. Mills† against

\* Phil. Mag. [IV.] vol. xliii. p. 503.

† Laboratory, 1867, p. 6.

the atomic (and therefore, it appears to me, against the molecular) theory of matter is, that this theory is not a result of inductive reasoning. An objection, in principle the same, is stated in his paper already referred to (*Phil. Mag.* Jan. 1876), in these words (he is speaking of the examination of natural phenomena):—"Instead of demanding a theory, it would be better to ask how we could dispense with one. It is juster and wiser to adhere to facts than attempt to transcend them."

In making such statements as these, it appears to me that Dr. Mills has misapprehended the method by which science gains her finest results. That method, I would make bold to say, is not entirely an inductive one; it makes use likewise of deductive reasoning. Science begins with facts; by observation and experiments she accumulates facts; by a proper use of imagination she frames hypotheses to account for the facts; and by subsequent appeal to facts she proves or disproves the truth of her hypotheses. Had science dealt with facts by strictly inductive methods only, she would never have been able to rise to those general expressions which we call laws. Hypotheses must be framed if science is to make any advances. Given a large number of facts, it might be possible, by arranging these in all possible combinations, to arrive at last at a proper classification; but this method is utterly impracticable, because of the vast number of data to be dealt with. It may be affirmed with a considerable amount of certainty that every great advance in science has been made by the use of hypotheses. Newton professed not to deal with hypotheses; but what is the '*Principia*' but the record of wonderful discoveries which became possible only by the greatest freedom in theorizing? Kepler theorized; Hooke asserted that the natural philosopher must be ready to guess the solution of many phenomena; Sir Humphry Davy did not despise hypotheses; and who made freer use of his imagination than Faraday?—the greatest of all experimental philosophers. In framing hypotheses we must see that they agree with facts; in other respects they may be as inconceivable (not self-contradictory) as any fairy tale. The suppositions involved in the theory of gravitation, and in the undulatory theory of light are seemingly sufficiently absurd; in the latter theory we are compelled to imagine the existence throughout space of a so-called æther, possessed of the highest elasticity, and at the same time more solid than steel! Dr. Thomas Young even imagined (and it would appear that we cannot deny the possibility of his imaginings being true) "that there may be independent worlds . . . pervading each other, unseen and un-



known, in the same space" (I quote from Prof. Jevons's work on the *Principles of Science*). Provided that a theory does not go against the primary laws of thought and of Nature, we must accept it, however absurd it may appear, if it be in accordance with facts. But in applying the deductive method of scientific reasoning, we are apt to forget that a theory, however perfect and satisfactory it may appear, is to be put to the severest test; it is only by showing that the known facts are explained by the theory, by foretelling facts which must be facts if the theory be true, and again by showing that these are indeed true, that a scientific theory can pass successfully into the number of accepted hypotheses. Newton's method of treating the theory of gravitation, and Faraday's method throughout his magnificent series of '*Experimental Researches*,' are typical examples of the true positions of Theory and Fact in scientific investigation. If it can be shown, therefore, that the molecular theory is not in accordance with facts, or is self-contradictory, it must be discarded; but to refuse to accept it because it has not been gained by purely inductive reasonings is simply, it appears to me, to show an ignorance of the true method of scientific investigation.

26. In examining the position of the molecular theory of matter as a scientific hypothesis, I would inquire:—Is it in accord with known facts? does it enable us to explain what would else appear to be disconnected phenomena? and are we able by its aid to predict phenomena which are afterwards proved to be true? In the following statement concerning the physical proofs of the molecular theory, I have drawn largely from Prof. Clerk Maxwell's '*Treatise on Heat*.'

27. The molecular theory is in accord with known facts concerning the condition of hot bodies.

A hot body possesses a certain amount of energy. This energy is capable of being changed from potential into kinetic energy; but such a change involves motion of some kind. When a hot body loses heat by radiation, this radiation must be effected either by the motion of material particles from the hot body to the body which receives the heat, or by the motion of some medium which fills the space between the body giving out and the body receiving heat; hence the particles on the outer surface of a body radiating heat must be in a state of motion. But hot bodies are continually radiating heat; therefore their outer particles must be in a state of continual motion; hence part, at least, of the energy of a hot body exists in the form of kinetic energy. But the motion of the particles of a hot body cannot be the motion of the body as a whole; otherwise the motion would be visible to us; it

must be the motion of exceedingly small parts: hence arises the conception of molecules, *i. e.* of small parts which remain intact during the movements which each part makes when the mass of the body to which it belongs is hot.

28. Again, the molecular theory is in accord with known facts concerning diffusion. If two gases be arranged in distinct strata, it is found that in a short time the presence of that gas which was at first confined entirely to the lower stratum is perceptible in the upper stratum, and *vice versa*; further, it is found that if a stratum of gas be issuing through another gas which is at rest, the movement of the former tends to communicate itself to the latter, which, in its turn, reacts upon the moving gas, tending to bring it to rest. Again, if the upper stratum of a gas be heated, it is found that the heat is communicated to the lower stratum. We have here, then, three kinds of diffusion—diffusion of matter, diffusion of momentum, and diffusion of energy. As we have learned to think of small parts of a body in a state of motion, we may extend the idea to the phenomena of diffusion, and imagine the little parts of the two diffusing gases becoming mingled together in the diffusion of matter; we may imagine the particles of the moving stratum of gas passing upwards and downwards into the surrounding strata of gas at rest, and hence having their motion partly checked, while they in turn communicate somewhat of their motion to the particles of the surrounding gas; and we may imagine, finally, the communication of heat from one part of a heated gas to another by the diffusion of the heated particles of the gas. But the rates of these diffusions are slower in liquids than in gases, especially the rate of the diffusion of matter, which is slowest of all (if, indeed, it takes place) in solids. Why is this? Because these little particles or molecules (their existence being granted) have not in solids the same freedom of motion which they have in liquids, in which, again, they have less freedom of motion than in gases. Although the particles of solids are not possessed of the same freedom of motion as is granted to those of liquids, yet we can easily see how there may be a rapid diffusion of energy from particle to particle, and hence can understand the fact that rapid diffusion of heat through solids is so often noticed.

29. Again, the molecular theory is in accord with known facts concerning evaporation and condensation.

If we grant that a gas consists of a number of molecules in a state of motion, we must believe that these molecules will from time to time come into collision; hence, even if their initial velocity be the same, an inequality of velocity will be produced. In a liquid the average velocity of the molecules

must be smaller than the average velocity of the molecules in a gas; nevertheless this does not prevent us from believing that the velocity of individual liquid molecules may be greater than that of individual gaseous molecules. Supposing that liquid molecules of such great velocities are moving at the surface of the liquid *from* the liquid, they will tend to escape from the liquid into the surrounding vapour. Supposing that a gaseous molecule of great velocity strikes the surface of the liquid, it will tend to become entangled in the liquid. Hence we shall have evaporation of the liquid and condensation of the vapour going on continuously.

30. So also I might go on to show that many of the facts of spectroscopy are in keeping with the molecular theory—that the vibration of each molecule may well be supposed to communicate itself to the ether in a regular manner, that an increase in density of a gas will cause the introduction of irregular motion among the molecules, and hence the appearance of a more or less continuous spectrum; but, for a consideration of the bearings of the molecular theory of matter upon the explanation of spectroscopic phenomena, I must refer to a lecture by Mr. J. Norman Lockyer, reported in ‘Nature,’ vol. x. p. 69.

31. In those points which I have briefly touched on, the molecular theory is, it appears to me, not only in accord with known facts, but it also enables us to group together and so to explain what had appeared to be isolated phenomena. But this theory does more than this: we can deduce from it certain conclusions which may then be proved true by experiments. Thus the law of Boyle, the law of Charles, and the law of Gay-Lussac may be deduced from a consideration of the dynamical conditions which must prevail among a group of particles supposed to constitute the mass of a gas, the pressure and temperature of which vary from time to time.

32. It has been possible to predict certain results from a consideration of phenomena viewed in the light of the molecular theory, which results have afterwards been verified. Thus the rate at which diffusion of energy must take place through air when one portion is heated was predicted from data derived from experiments on viscosity. The actual measurement of the diffusion-rate, made by Prof. Stefan, of Vienna, has proved to be in close agreement with the predicted rate.

33. If we interpret chemical notation in the light of the molecular theory, we assume that the symbol of each chemical substance represents a molecule of that substance, and that chemical equations represent the results of the mutual actions

of molecules. The relative weights of molecules are therefore assumed to be represented by our chemical symbols. In order to deduce such weights with accuracy, we are obliged to determine the relative densities of the substances in the form of gas, and to assume the truth of the law of Gay-Lussac or of Avogadro. But this law can be deduced from the molecular theory of matter; hence, when we speak of "molecular weights," we assume the truth of the molecular theory; and I therefore think that it behoves us to know what this theory really is, and on what physical grounds it can be upheld.

34. I have already stated that the ordinary chemical notation does not appear to me to necessitate the assumption of the existence of molecules; but, granting that matter has a molecular structure, this notation will readily lend itself to the needs of such a theory. Can we, however, with equal facility explain the general phenomena of chemical action in terms of the molecular theory of matter?

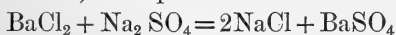
35. The definition of a "molecule" quoted from Prof. Clerk Maxwell's work does not prevent one from imagining an inner structure for such molecules. We may think of one of these little parts which holds together while the body to which it belongs is hot as made up of a greater or less number of still smaller parts, parts either of the same or of different kinds of matter. In the conception of such smaller parts we have the idea of chemical atoms. Now it may legitimately be supposed that during the excursions which molecules are ever making, some of them will be so knocked about as to have their atomic structure disarranged; under ordinary circumstances this disarrangement will quickly be restored; but if a new force be called into play which shall act by increasing the tendency to molecular disaggregation, we shall have a more or less complete decomposition of the original molecules, attended, of course, with the production of new molecules. The electrolysis of liquids may be regarded as a directive action exercised by electromotive force upon those molecules which have undergone disaggregation by clashing together, whereby one component part of the disaggregated molecule is directed in one direction, and another in another direction.

36. So also the phenomenon of dissociation meets with an explanation in terms of the molecular theory by supposing that at high temperatures the clashing of molecules is carried on so violently that more molecules suffer decomposition in a unit of time than are able to regain their original structure. Dissociation is then explained as a "change in the configuration and motion of a material system."

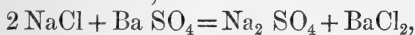
37. If this view of intramolecular action be adopted, we

must regard chemical action as constantly taking place. To use Dr. Mills's words (*Phil. Mag.* Jan. 1876), "Chemical action can begin, because it never has ceased."

38. Suppose, however, that the molecules of a given substance (A) come into contact with those of another substance, (B), and that these substances are capable of exercising chemical action upon each other, we shall have an interchange of "atoms" between the molecules A and B; at any moment, therefore, we shall have four kinds of molecules present—A and B, the generating substances, and A' and B', the products. We shall have an exchange of atoms taking place between A and B, an exchange of atoms taking place between A' and B' (and possibly an exchange of atoms taking place between A and A', and between B and B' &c.). Now the final condition of atomic distribution among the reacting molecules will depend chiefly upon the rate of atomic interchange; if more changes take place between the atoms constituting the molecules A and B than between the atoms constituting the molecules A' and B' in equal times, it is clear that an increase in the number of the molecules A' and B' will take place; and conversely, if more atomic changes take place between A' and B' than between A and B in equal times, the number of A and B molecules will increase; in other words, the decomposition of A and B will cease. Again, if by any means it is possible to remove the newly formed molecules A' and B' from the sphere of action, then the atomic interchange represented by the equation  $A + B = A' + B'$  will proceed until A and B have entirely disappeared, and the sum of the weights of A' and B' is equal to the original sum of the weights of A and B. Such an action as this very generally takes place in ordinary chemical reactions. For instance, the equation



represents the final atomic arrangement of the molecules taking part in the reaction; but we must imagine that a secondary reaction, symbolized thus,



also takes place, but only to a very limited extent, because the molecule  $\text{BaSO}_4$  is removed from the sphere of action almost as quickly as it is produced. It is true, as has been urged, that the equation  $\text{Zn} + \text{H}_2 \text{SO}_4 = \text{ZnSO}_4 + \text{H}_2$  has never been rigorously realized in practice. Why? Because, before the atomic interchanges symbolized are completed, so many molecules having the composition  $\text{ZnSO}_4$  have been formed, and have *not* been removed, that the completion of the change becomes impossible. Remove the products of the action, how-

ever (as is actually done in general practice), and the equation represents the final atomic distribution. Yet, although the equation does not express everything that occurs when zinc and sulphuric acid are mixed together, it does express the great fact that a definite quantity of zinc is invariably associated with definite quantities of sulphur and of oxygen in the production of the substance called sulphate of zinc. (For a further discussion of this subject, see the address of the President, Dr. Williamson, to the British Association at Bradford, 1873.)

39. Further, such atomic interchanges will involve changes in the form of the energy of the reacting molecules: potential will be converted into kinetic energy, or *vice versa*. Such changes will be marked by absorption or by evolution of heat. These energy-changes will certainly bear a close relation to the general character of the products of the reaction. Thus evolution of heat (stoppage of molecular motion of some kind) is accompanied with the production of a substance whose "affinity-value" and whose boiling-point are higher than those of the generating substance (Wright, Phil. Mag. Dec. 1874).

40. Now this change in the form of energy will be dependent upon a change in the relative position of the parts of the material system, the energy of which is changed; hence the mere fact of "heat-disturbances" taking place in chemical reactions is in perfect keeping with a molecular theory of matter.

41. But certain special cases of heat-disturbance have been cited as being apparently inexplicable in terms of the molecular theory. Thus Dr. Wright\* brings forward the fact that there is a change in the form of energy of the material system accompanying the exchange of matter symbolized, by the equation  $H_2 + Cl_2 = 2HCl$  as opposed to, or at least as being probably incapable of satisfactory explanation by, the molecular theory. He holds that, "granting that the substitution of chlorine for hydrogen is attended by a given change of motion, the inverse substitution must be accompanied by the opposite change." He supposes that in the reaction under notice, an atom of chlorine in the chlorine molecule is replaced by an atom of hydrogen, while an atom of hydrogen in the hydrogen molecule is replaced by an atom of chlorine, and that "those complementary changes should cause, on the whole, no difference as to the ultimate amount of motion in the two original and the two resulting molecules." It appears to me that Dr. Wright has here assumed that two equal and opposite actions, called by him "substitution" and "inverse substitution," are taking place. It would, I think, be more

\* Phil. Mag. [IV.] vol. xliii. p. 511.

in accord with the molecular theory to suppose that there is taking place so rapid an atomic interchange among the molecules both of hydrogen and of chlorine that when these two substances are brought together, under suitable conditions, it is easy for the momentarily dissociated atoms to combine together so as to form the new molecules of hydrochloric acid, and that when these are formed there is not so rapid an atomic exchange taking place among them as there is among the molecules of the generating substances. The general result of such an action, so far as change of energy is concerned, would therefore be an evolution of heat. Indeed the fact that it is possible to completely carry out the reaction symbolized above, appears to me to be in perfect keeping with the fact of the evolution of heat which is noticed during the the reaction; for if the number of atomic exchanges between the products  $\text{HCl HCl}$  were as great as those between the generating molecules  $\text{H}_2 \text{Cl}_2$  in equal times, the decomposition would cease before the equation was completely realized. But the actual result points to the conclusion (on the assumption that the molecular theory is true) that there are less rapid intramolecular movements occurring in the case of the molecules  $\text{HCl HCl}$  than in the case of the molecules  $\text{H}_2 \text{Cl}_2$ —in other words, that some portion of the initial molecular motion has disappeared. But inasmuch as the total energy of the system is supposed the same, the energy which has disappeared in one form must have reappeared in another; it *has* reappeared as heat.

42. Lastly, as to isomerism. So called isomeric bodies undergo different reactions; they are produced by different reactions, &c.: these facts may be expressed irrespective of any theory as to their cause, by means of structural or dissected formulæ. It has been supposed that isomerism is to be explained by imagining that the atoms are differently arranged in isomeric molecules. Again, it has been supposed that isomerism is to be explained by granting that isomeric bodies are endowed with varying amounts of energy, that in the formation of these bodies different quantities of energy change form, and that, conversely, the various reactions noticed with isomeric substances are to be traced to different degrees of conversion of one form of energy into another. Dr. Mills (*loc. cit.*) says, "to measure, both in quality and in quantity, the energy associated with each isomeric substance, is to give a complete account of the phenomenon of isomerism." Not, it seems to me, to give a complete *dynamical* account of the phenomenon of isomerism; for it is only when we explain a phenomenon as a "change in the configuration and motion of

a material system," that we give a complete dynamical explanation of that phenomenon. If Isomerism depends on changes of potential energy, we must remember, to quote Prof. Clerk Maxwell (Treatise on Heat, p. 283, 1st edit.), "that potential energy depends essentially on the relative positions of the parts of the system in which it exists, and that potential energy cannot be transformed in any way without some change of the relative position of those parts."

Hence I think that the energy theory of isomerism is not altogether inconsistent with the position-theory, and that both meet with their fullest development in terms of the molecular theory of matter.

43. I have thus attempted to show that our ordinary chemical notation is founded on facts, that it gathers together and symbolizes a great many phenomena, that it does not necessarily imply the acceptance of any theory as to the constitution of matter, but that it receives its fullest meaning when viewed in the light of the molecular theory. I have further briefly endeavoured to point out some of the deficiencies of chemical notation, but have expressed a belief that when the present system is superseded by a better, the new will be the legitimate outcome of, and will not be in any fundamental point opposed to the old.

II. *On a new Form of Polariscopes, and its application to the observation of the Sky.* By R. H. M. BOSANQUET, Fellow of St. John's College, Oxford\*.

THE observation of faint traces of polarization, such as present themselves in certain atmospheric and other phenomena, requires polariscopes of great sensibility. The instrument hitherto principally employed for the purpose is the polariscopes of Savart, formed of two similar plates of quartz, cut at an angle of  $45^\circ$  with the axis, and superposed with the principal sections at right angles. The appearance presented by polarized light in this instrument, when properly arranged, consists of a number of coloured bands with black or white centre; and its sensitiveness depends on the fact that these bands can be recognized even when very faint, on account of the marked character of the pattern they form in the field of view. In one respect, however, this instrument possesses properties which are not always advantageous. First, it is necessary that the principal section of the analyzer between the plates and the eye should be either parallel or at right angles to the direction

\* Read before the Ashmolean Society, Oxford, May 25, 1876, and communicated by the Author on behalf of the Society.



of the bands. At an intermediate position, where the angle between these directions is  $45^\circ$ , no effect is visible. Secondly, it is necessary that the direction of the bands should be either parallel or at right angles to the plane of polarization of the incident light; at an intermediate position, where the angle between these directions is  $45^\circ$ , no effect is visible. This latter property may be expressed by saying that, with this polariscope, the transition from positive to negative polarization takes place by passing through the value zero, positive and negative being a conventional mode of expressing two directions of polarization at right angles to one another.

Now although this property of Savart's polariscope has its advantages, yet in the preliminary examination of regions of faint polarization, where the existence and direction of polarization are both uncertain, it is extremely inconvenient. In cases where the polarization is so faint that it can only be seen by glimpses after intense gazing under the most favourable circumstances, it is difficult to map out the distribution of the intensity where it is thus complicated with the question of direction; for at the moment when one is certain that no bands are seen, it may be that there is a polarization in a plane inclined  $45^\circ$  to the bands; and the process of rotating the instrument necessary to convince oneself of the real character of the phenomenon adds much to the difficulty of the observation. The polariscope, of which I proceed to give an account, has been devised with the object of removing this difficulty; it gives bands nearly (if not quite) as distinct as those of the Savart; and the presence of the bands is independent of the position of the analyzer, and of the plane of polarization of the observed light.

The new polariscope consists essentially of two wedges of quartz, the one right-handed and the other left-handed. The terminal faces are cut at right angles to the axis; and the inclined common surface of the wedges makes an angle of about  $30^\circ$  with the direction of the axis, which is also the line of vision.

In the larger instrument which I have constructed, the length of the parallelopiped which is made up of the two wedges is about 1.5 inch; and the section is about .9 inch square. In the smaller instrument (which was constructed first, by way of trial) the angle of the wedge is about  $45^\circ$ , the length between two terminal faces nearly half an inch, and the dimensions of the area seen through are .8 inch parallel to the bands, by .45 inch. This latter instrument was made out of two small crystals bearing right-handed and left-handed hemihedral faces, which I selected from my collection of such

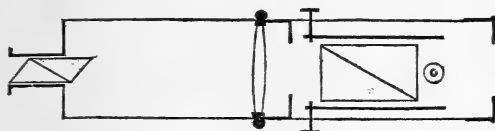
specimens procured in Switzerland. The crystals proved to be full of striæ; but notwithstanding this, the appearance of the bands is substantially perfect when the line of vision coincides pretty closely with the axis. This is an observation worth noting; for it seems probable that most of the Swiss quartz is so striated as to make it useless for many purposes, but that the striated structure is so situated as not to affect vision in the direction of the axis. I possess some fine Swiss crystals with well-developed hemihedral faces, which present external markings similar to those on the crystals which proved striated: if such as these can be used for the present purpose, the expense of perfect crystals of the requisite size may be avoided; for any one can pick up in Switzerland for a few francs large and fine specimens of this kind.

The small instrument of Swiss quartz is mounted at the end of a tube 10 inches long with a small Nicol's prism interposed between it and the eye. It gives, with polarized mean yellow light, three complete bands, and already forms a very useful and sensitive polariscopes for ordinary purposes.

By reducing to millimetres, taking the rotation of the mean yellow ray at  $24^\circ$  for 1 millim., and remembering that a parallelopiped of this kind made up of two opposite quartz wedges exerts a rotatory effect due to twice its depth, we can show that every complete band in the mean yellow requires a depth of  $\cdot 15$  of an inch from back to front. To get a more extensive series of bands I procured two fine pieces of quartz, right-handed and left-handed respectively, out of which the compound quartz first described, viz.  $1\cdot 5$  inch long and  $\cdot 9$  inch in section, was constructed. According to the above calculation this should give ten bands with yellow polarized light. A rather small stop has been inserted. But within this I can count eight bands when the instrument is directed to the reflection of a salted spirit-flame in a polished table. Of course with white light the systems of bands belonging to the different colours are soon superposed, and only about five or six bands are prominent. But the central bands, whether white or black, are sharply defined.

The bands of this compound quartz are delicate and beautiful when it is held at some distance and looked at through a Nicol with polarized light; but it was my object to secure sensitiveness by bringing it nearer the eye so as to get more light; and also it was necessary to have it mounted with an adjustment, by which the axis could be placed parallel to the line of vision. The form adopted is illustrated in the accompanying figure. Parallel rays, after traversing the axis of the compound quartz, fall upon a simple lens of 4 inches focal length

and 1 inch diameter, pass through a small Nicol's prism, and then fall upon the eye, which is placed in the focus of the lens. Fixed or movable micrometer wires may be introduced just beyond the end of the quartz, for the purpose of measuring the displacement of the bands, an indication of the direction of the plane of polarization; but I have not introduced these. The region indicated, however, is in focus, and the focus is deep.



The quartz is adjusted for the axial direction by two pairs of screws. The one pair is seen in profile; the other pair is at right angles to the first, and has one of the round heads indicated.

Scale  $\frac{1}{2}$ .

I remarked at the outset that the difficulty in connexion with Savart's polariscope might be expressed by saying that it passes from positive to negative polarization through a zero value. The polariscope now described, on the other hand, may be regarded as passing under the same circumstances from functions of a certain angle to functions of an angle differing by  $90^\circ$  by simple progression, the intensity of the appearances manifested being the same throughout. For the effect of passing through a certain thickness of the crystal is to turn the plane of polarization through an angle proportional to the thickness; and the same difference of thickness will always correspond to  $180^\circ$  of rotation, or to the interval of one band of a given colour. Hence the appearance will always be that of a succession of bands.

To determine what appearance of motion in the bands is produced by given rotations of polarizer or analyzer, it will be convenient to imagine a section mapped out with lines drawn at intervals corresponding to 1, 2, ... millims. thickness of right-handed and left-handed action respectively. As an example we will take polarized homogeneous yellow light, in which the millimetre lines correspond to rotations of  $24^\circ$ ; and we will suppose that the analyzer is turned round through successive angles of this amount in the direction of the hands of a watch facing the observer, *i. e.* left-handed in the direction of the ray. In the left-handed quartz, after each such rotation, the corresponding light is that which comes through 1 millim. of increased thickness; consequently the bands appear to move from the centre. In the right-handed quartz,

on the contrary, the action of diminished thickness corresponds to left-handed rotation, and the bands appear to move to the centre. Thus for rotation of analyzer with watch the bands move from the right-handed quartz to the left-handed one, and *vice versa*.

In white light, as the breadth of one band is different for each colour, and the rotation of  $180^\circ$  in either the incident polarization or the analyzer displaces every colour through one band, the effect of rotation on the bands is complex; but they remain substantially equally visible in all positions\*.

*Application to the observation of the sky.*

In a paper read before the Ashmolean Society in 1875†, I pointed out the untenableness of the conception of neutral points as separating the positive and negative regions of polarization in the sky, and suggested that it was probable for various reasons that these two regions were separated by a ring in which the polarization passed through a value zero. At that time I had not succeeded in observing the polarization in the neighbourhood of the neutral points at all. Great difficulty was found, even with the Savart, in settling the point; and the polariscopes above described were constructed for this purpose. With the assistance of this and the Savart, which supplement each other's indications in a useful manner, I have made out the state of the facts.

The tract which separates the regions of positive and negative polarization exhibits, elsewhere than in the neutral point, polarization of sensible amount, which is neither positive nor negative, but is in planes inclined at angles other than right angles to the plane through sun, point, and observer. That is to say, the direction of the polarization changes from that plane to a direction at right angles with it by a gradual rotation through the right angle, and not by passing through a zero value, unless we cross over the neutral point itself.

The first fact in support of this statement is, that it has been ascertained beyond a doubt by the new polariscopes that the polarization is continuous between the positive and negative regions; *i. e.* the bands never vanish while the instrument is slowly passed from a point in the negative region to a point in the positive one, unless we cross over the neutral point itself.

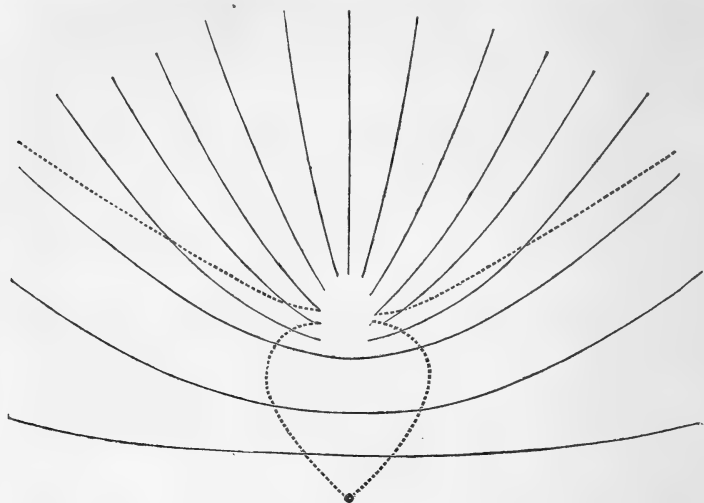
The property of the Savart then comes in usefully—that it

\* The instrument was constructed by Messrs. Tisley and Spiller. It is now in the Loan Exhibition of Scientific Instruments at South Kensington.

† Phil. Mag. Dec. 1875, Suppl.

gives no bands where the polarization is inclined  $45^\circ$  to the direction of the bands. Now when the neutral point is regarded with the Savart under favourable conditions, a neutral line is seen to extend across the bands, if they are vertical or horizontal, in a shape somewhat resembling an hyperbola, with the vertex at the neutral point, and the convexity turned downwards; but there is always a portion at the vertex which looks like a horizontal neutral line, presenting the appearance figured in Brewster's paper (Edinb. Trans. 1861, p. 213). In particular the neutral line is horizontal, or nearly so, where the part of it due to the property of the Savart joins the true neutral point. Under these circumstances I am able to say with certainty that the polarization in the region immediately adjoining the neutral point at its side is always in a plane inclined  $45^\circ$  to the vertical nearly. I then employed both polariscopes to find out what the actual directions were; the results are expressed in the following sketch, which also embodies the general distribution throughout the region in question, as derived from a considerable number of observations of Arago's and Babinet's points. The distribution about Arago's point and the antisolar point is similar to that about Babinet's point and the sun, as Brewster made it. I have not attempted to observe Brewster's point for this purpose, as the observations are trying to the eyes when the sun is high enough to see it. The continuous lines in the figure give approximately the positions of the plane of polarization. The hyperbolic dotted line represents the neutral curve seen with a Savart with bars vertical or horizontal; it is the locus of points where the direction-lines are inclined  $45^\circ$  to the vertical. The neutral point is in the middle, the sun or antisolar point is represented by the dot below.

It is a matter of some interest to inquire what is the figure of the tract which separates the positive and negative regions. We may for this purpose draw the locus of points at which the directions of polarization make an angle of  $45^\circ$  with the radius vector to sun or antisolar point; the resulting locus may be regarded as a sort of diameter of the tract in question, which extends on both sides of it. It forms a curve like a loop of a lemniscate, having its centre at the sun or antisolar point, and touching the vertex of the Savart hyperbola with the flat end of the loop, if we imagine the two curves continued through the neutral point, of which their common vertex is the centre. It is drawn in dots in the lower part of the figure.



Distribution of direction of planes of polarization about a neutral point lying above sun or antisolar point. The sun or antisolar point is indicated by the dot at the bottom of the figure.

*Outline of the theory of the distribution of polarization in the neighbourhood of a neutral point.*

If two equal polarizations be superposed whose directions make with each other any angle, the resulting direction of polarization lies along the bisector of the angle between them.

Let  $A$  be the common amplitude of the two vibrations,  $\theta, \phi$  their directions estimated from any initial position,  $\gamma$  the angle between them; then  $\theta - \phi = \gamma$ .

Let  $X$  be the intensity of the components along the initial line. Then  $X^2 = A^2 (\cos^2 \theta + \cos^2 \phi)$ . It is required to find the positions for which  $X$  has maximum and minimum values.

$$X \, dX = 0 = A^2 (\cos \theta \sin \theta \, d\theta + \cos \phi \sin \phi \, d\phi);$$

and

$$d\theta - d\phi = 0,$$

whence

$$\begin{aligned} \sin 2\theta &= -\sin 2\phi, \\ \theta &= -\phi, \end{aligned}$$

or

$$\theta = \pi - \phi, \text{ whence } \frac{\theta + \phi}{2} = \frac{\pi}{2};$$

and the required positions are the internal and external bisectors of the angle between the given polarizations. It is evident that the internal bisector corresponds to the maximum, the external to the minimum value. If, then, a constant hori-

zontal polarization be combined with an equal one making a small angle  $2\alpha$  with the vertical, the resulting polarization will be inclined to the vertical at the angle  $45^\circ + \alpha$ , or nearly at the angle  $45^\circ$ .

We can thus account for the fact that the polarization at the side of the neutral point is inclined about  $45^\circ$ , by supposing that the polarization directed to the sun or antisolar point is combined with a uniform horizontal polarization of equal amount at the height of the centre of the neutral point; and the explanation admits of extension to the whole phenomenon.

The polarization due to the sun must obviously be considered as increasing regularly from sun or antisolar point to the great circle at right angles to the line joining them. This polarization must therefore be in the first stage of Tyndall's experiments—not in a more advanced one, as supposed in my former paper; the imperfect polarization at right angles to the sun must arise from some other cause.

The question then is, to what is the horizontal polarization in the neighbourhood of the horizon due?

The most obvious answer is that which Brewster gave: it seems as if it might be due to polarization by refraction through the atmosphere, in the same way that light is polarized by oblique refraction at a surface of glass. Brewster, however, did not compute the value of this polarization, which the astronomical-refraction tables permit us to do.

It is commonly said that the maximum polarization by refraction takes place at the polarizing angle. But this is not true. If we consider the Brewster-Fresnel formula,

$$\frac{\text{ratio of components after refraction}}{\text{ratio of components before refraction}} = \frac{1}{\cos(i-r)},$$

we see that the polarization by refraction increases up to the limiting incidence\*.

To calculate the maximum polarization producible by horizontal refraction through the atmosphere regarded as homogeneous, we can suppose the incident light unpolarized, and put  $i-r=33'$ , which is the value of the astronomical horizontal refraction.

If we then calculate the resulting value of  $R$  (where  $\sin 2R = \text{polarization}$ ), we find that it amounts only to a few seconds of arc.

\* See Jamin, iii. 2. p. 679, where it is assumed that because the *difference* of the intensities of the components has a maximum, the polarization has one; but this does not follow.

We should expect such a polarization to be insensible; and that it is so is confirmed by the observation of the setting sun with the polariscope which I have succeeded in making. No trace of polarization is to be seen in the sun's light.

Let us see what the positive polarization at the neutral point will be if we compute it on the supposition that it arises from the simplest case of diffraction, as is most probably the case.

Let A, B be the amplitudes which would produce the intensity and polarization observed normally to the sun, so that the polarization there is  $\frac{A^2 - B^2}{A^2 + B^2}$ ; and put  $\frac{B}{A} = \tan \phi_n$ . Then the polarization of the same components looked at in a direction forming an angle  $\alpha$  with the line to the sun is

$$\frac{A^2 (1 - \cos^2 \alpha) - B^2 \sin^2 \alpha}{A^2 (1 + \cos^2 \alpha) + B^2 \sin^2 \alpha}.$$

Comparing this with the form

$$\frac{\cos^2 \phi - \sin^2 \phi}{\cos^2 \phi + \sin^2 \phi} = \cos 2 \phi = \text{polarization},$$

we have

$$\tan^2 \phi = \cos^2 \alpha + \tan^2 \phi_n \sin^2 \alpha.$$

In the sky  $\phi_n$  is generally from  $20^\circ$  to  $25^\circ$ ; and putting  $R = 45^\circ - \phi$ , we find for R values between  $1^\circ$  and  $2^\circ$ .

These values are so much larger than that due to horizontal astronomical refraction that it seems impossible that the two can compensate each other at the neutral points.

We are thus thrown back on the original speculation of Arago, that the horizontal polarization in the neighbourhood of the horizon is due to the effect of light reflected from the atmospheric regions surrounding the observed point. It is clear that this is capable of furnishing an explanation; for the light diffused from sources in a line with the observer would not be specially polarized by diffraction at the observed point, while that from sources to the right and left would be horizontally polarized, as the plane of source, point, and observer would be in the mean horizontal. But it does not seem practicable to establish any definite theory of this kind. The principal interest of this horizontal polarization and its effects would appear to be in connexion with the meteorological condition of the atmosphere.



III. *On the Nebular Hypothesis.*—III. *Our Binary Star and its attendants.* By PLINY EARLE CHASE, *Professor of Philosophy in Haverford College.*

[Continued from vol. i. p. 510.]

IN studying the special evidences of nebular action we find various significant relations, based on the following cardinal planetary positions, for which Stockwell's\* values are taken :—

	Secular perihelion.	Mean perihelion.	Mean.	Mean aphelion.	Secular aphelion.	Secular range.
♂	·29743	·31873	·38710	·45546	·47667	·17934
♀	·67224	·69779	·72333	·74888	·77442	·10218
♂	·93226	·96613	1·00000	1·03387	1·06774	·13547
♂	1·31105	1·40322	1·52369	1·64416	1·73633	·42528
♂	4·88632	4·97824	5·20280	5·42735	5·51927	·63295
♂	8·73415	9·07764	9·53885	10·00006	10·34325	1·60880
♂	17·68803	18·32298	19·18358	20·04418	20·67913	2·99111
♂	29·59817	29·73221	30·03386	30·33551	30·46955	·87138

1. The minimum eccentricities of the principal planets, as found by Stockwell, are :—Neptune, ·00557 ; Uranus, ·01176 ; Saturn, ·01237 ; Jupiter, ·0255 ; Earth and Venus, each, 0. The ratios, counting towards Sun, are, therefore,  $\Psi : \delta = 1 : 2·11$  ;  $h : \gamma = 1 : 2·07$ . The closeness of these approximations to the fraction  $\frac{1}{2}$  suggests their probable dependence on a fall through a half-radius, which would give the particles of a nebulous ring the velocity of separation.

2. The secular ranges of the planets preserve many suggestive features. Jupiter's (·63295) corresponds with Earth's orbital radius of spherical gyration (·63245) ; Saturn's (1·6088) with the nucleal tendency of Earth's kinetic radius (1·4232 $\frac{3}{4}$  = 1·6009) ; Uranus's (2·99111) with the asteroidal belt, and with a linear pendulum of which Earth occupies an oscillatory centre ; the range-sum of Neptune and Earth (1·00685) with Earth's mean vector radius, of Venus and Mars (·60462) with the kinetic atmosphere (·60087) ; the sum-ratio of Earth and Venus (·23765) with the ratio of Mercury's greatest eccentricity (·23172).

3. Stockwell's estimates for the maximum secular eccentricity, Bessel's for the masses of Jupiter and Saturn, and Newcomb's for those of Uranus and Neptune, give the following values for the positions of centres of gravity, at secular perihelion, mean, and secular aphelion, the unit being Sun's radius :

\* Smithsonian Contributions, 232, pp. 37, 38.

	Perihelion.	Mean.	Aphelion.	Approximate ratio.
☉ $\gamma$	1.0019	1.0668	1.1318	1
" $h$	.5360	.5883	.6347	$\frac{1}{2}$
" $\delta$	.1681	.1824	.1966	$\frac{1}{3}$
" $\psi$	.3228	.3276	.3323	$\frac{1}{5}$

These values likewise exhibit a close approximation to the perihelion ratio  $\frac{1}{2}$ , between the comparison planets of each pair, together with indications of nebular rupture between Saturn and Uranus, and of increasing condensation towards Neptune and Jupiter. The perihelion-ratio of Neptune : Jupiter = 1 : 3.1 ; that of Uranus : Saturn = 1 : 3.2, or nearly that of a nodal division to the entire length of a linear pendulum. The reversal in the direction of condensation, between the central and the exterior belt, may perhaps explain the retrograde satellite-motions of Uranus and Neptune.

4. If we compare the perihelion and aphelion centres of gravity of companion planets, we find that .6347 is near the centre of spherical gyration ( $\sqrt{4} \times 1.0019 = .6336$ ) of 1.0019, and that .1681 is near the centre of nebular rupture\* (.1661) of .3323.

5. The time of revolution varying as  $r^{\frac{3}{2}}$ , while the time of rotation with the velocity due to interior *vis viva* varies as  $r^{\frac{1}{2}}$ , the limiting radius of synchronous rotation and revolution, for any given expanding or contracting nucleus, is a mean proportional between the limiting radii of interior and exterior nucleal rupture. I have shown that the gravitating impulses are  $4565 \times 10''$  per second, corresponding in frequency with the red rays of light; and the modulus of light is  $\left( \frac{365.256 \times 86400}{252 \times \sqrt{214.86 \times 497.825}} \right)^2 = 473755$  solar radii. If modulus were taken as the primitive radius of resisting inertia ( $r^{\frac{3}{2}}$ ), Neptune's position would accord with the corresponding nucleal radius ( $r = 6077.2$  solar radii), and Mercury's with the radius of internal rupture ( $r^{\frac{1}{2}} = 77.96$  solar radii). Saturn's place being fixed, as we have seen, by the centre of nucleal planetary inertia, its *mean* aphelion radius appears to have influenced Neptune's position, while Earth's *secular* aphelion exerted a like influence on Mercury's position; for  $1.04835 \times 6077.2 = 6371$ , Neptune's secular perihelion being 6359.5, a difference of less than  $\frac{1}{5}$  of one per cent.;  $1.06774 \times 77.96 = 83.24$ , Mercury's mean distance being 83.17, a difference of about  $\frac{1}{12}$  of one per cent.

\* Velocity at  $\frac{1}{2} r = \sqrt{2} \text{ gr.}$

6. The mean proportional between these values of  $r^{\frac{1}{2}}$  and  $r$ , as well as between Sun's radius and modulus, is 688.3 solar radii, or 3.203 times Earth's mean radius vector, which is near the outer limit of the asteroidal belt. This agrees very closely with the secular range of Uranus (2.99111); and the relation is still closer to the ratio between the gravitating radii of Saturn and Uranus  $\left(\frac{5883}{1824} = 3.209\right)$ . The positions of the principal masses in the three chief companion planetary pairs, indicate the same law of mean proportionality between interior and exterior rupturing tendencies. For Jupiter's secular aphelion (5.5193) is at the geometrical mean (5.5195) between Earth's mean distance and Neptune's secular aphelion (30.46955). Neptune's secular perihelion (29.598) is within one per cent. of  $6 \times$  Jupiter's secular perihelion, or in inverse ratio of the indices in my equation of products of figurate powers ( $\Psi^1 \times \mathfrak{S}^3 \times \mathcal{U}^6 = \mathfrak{h}^{10}$ ).

7. The nodal influence of linear centres of oscillation on material particles which are subjected to radial "lines of force" is shown in the vector radii of the three outer planets (Saturn, *mean aphelion*, 10; Uranus, *mean aphelion*, 20.044; Neptune, *mean*, 30.034). This tendency would be aided by the apparent primitive interposition of Sun between Jupiter and the exterior planets; for Saturn's vector radius is so small in comparison with modulus, that the above positions represent the geometrical progression  $(n+1)$ ,  $(n+1)^2$ ,  $(n+1)^3$ , as well as the arithmetical pendulum progression 1, 2, 3—thus satisfying the requirements both of elastic media and of simple force-lines. The many indications that Jupiter and Saturn were once parts of the same nebular belt, with a mean nodal difference\* of  $180^\circ$ , serve to connect these accordances with the figurate equation.

8. As further clues to the significance of the figurate equation, it may be well to note the closeness of the accordance between the mass-ratios  $\odot : \Psi$ , 6, or  $\odot : \mathcal{U}$ , and the distance-ratio  $(\mathcal{U} : \oplus)^6$ , as well as Stockwell's ratios\* of mean perihelion- and node-motion.

9. The radial light-oscillation which is synchronous with the present limit of possible circular revolution is  $10020^{\circ}25' \div 497.825 = 20.128$  Earth's vector radii, which is also the rupturing radius of the retrograde-satellite and the direct-satellite planets, the difference between the secular aphelia of Saturn and Neptune (or the vector radius of Neptune relatively to the nebular planetary centre of inertia) being

\* Stockwell, *op. cit.* p. xiv.

20.126; the mean aphelion vector radius of Uranus = 20.044; the major axis of the November meteors, and the secular aphelion of Uranus, each = 20.68; twice Saturn's secular aphelion = 20.69,—the original nebular activity thus combining with the satellite influences in maintaining Saturn's rings. Moreover Neptune's secular perihelion =  $1.4313 \times$  the secular aphelion of Uranus—the “kinetic radius” or the limiting radius of equality, towards which I have shown that *all* central forces mathematically tend, being 1.4232.

10. The sum of Uranus's mean and Earth's secular perihelion vector radii = 20.0158. The importance of Earth's position, the near approximation of this sum to the cardinal light-oscillation (9), and the indications of a somewhat shorter major axis for the inner meteors of the November stream encourage us to look for still further evidences of continuing nebular activity in our own orbit as well as in that of Saturn. If Earth and Uranus were once parts of an elliptical ring, or meteor-current, with Earth sharing Uranus's present maximum secular eccentricity (.077965), Earth, Jupiter, Uranus, and Sun were connected by the following equation:—Modulus =  $252 \times$  the square of Jupiter's mean radius vector  $\times$  the time of revolution at secular perihelion (.922035) of a mean-proportional radius between Earth and Sun ( $1 : \sqrt{214.86}^{\frac{2}{3}} \div$  (square of Earth's radius  $\times$  1 year). This value of modulus (474250) exceeds the value found by the ordinary methods (473755) by less than  $\frac{1}{5}$  of one per cent.

11. The secular range of Uranus, between 17.688 and 20.679, subjects all the intraasteroidal planets, together with most of the asteroidal belt, to the influence of its accompanying light-oscillations, so that all the members of our interior system of dense planets may have been partially built up of materials from a meteoric stream, of which the November meteors are the debris. Earth's secular aphelion (1.0677; cf. the ratios of  $\text{♃}$   $\odot$  centre of gravity, 1.0668, and of Jupiter's secular aphelion, 1.0608) was established near the linear centre of gravity of a pendulum of which the kinetic radius marked the centre of oscillation ( $\frac{1}{2} \times \frac{2}{3} \times 1.4232 = 1.0674$ ). Venus's secular perihelion (.6722) is near the centre of spherical gyration of Earth's secular aphelion ( $\sqrt{4} \times 1.0677 = .6753$ ).

12. If we take the primitive light-axis (20.128) and suppose it subjected to repeated oscillation through  $\pm$  Earth's mean vector radius, successive nodal bisections give the following approximations:—

$20.128 - 1 = \alpha \dots$	$19.128$	Uranus ...	$19.184$
$\frac{1}{2} \alpha = \beta \dots$	$9.564$	Saturn ...	$9.538$
$\frac{1}{2} (\beta + 1) = \gamma \dots$	$5.282$	Jupiter ...	$5.203$
$\frac{1}{2} (\gamma - 1) = \delta \dots$	$2.141$	Asteroids.	
$\frac{1}{2} (\delta + 1) = \epsilon \dots$	$1.571$	Mars ...	$1.524$

13. If the kinetic radius of Mercury's secular perihelion was also the radius of nuclear separation, its secular aphelion ( $102.438$ ) was near a corresponding atmospheric radius ( $1.4232^{\frac{1}{2}} \times 63.906 = 102.305$ ).

14. If the luminiferous or kinetic æther is the permanent representative of the original hypothetical nebula, in which suns and planets are but fleeting particles, Faraday's "lines of force" may be due to longitudinal and transverse waves, either of various degrees of velocity but uniform frequency, or of various frequency but uniform velocity, or of velocities and frequencies both varying, in accordance with definite harmonic laws. The luminous, thermal, magnetic, gravitating, cohesive, and dissociating impulses\* all have simple harmonic representatives in the 26th musical octave, which is the special octave of light-proper; the gravitating impulses have also important harmonics in the 46th, 91st, and 92nd octaves.

15. Jupiter, the chief planet in the supraasteroidal, and Earth, the chief planet in the intraasteroidal belt, are connected by the following proportions. The number of light-oscillations ( $\log = 20.699916$ ) which would communicate the greatest gravitating velocity in our system ( $\sqrt{2gr}$  at Sun) : the number ( $\log = 15.822542$ ) in describing Sun's circumference ( $2\pi r$ ) : : velocity of revolution at Earth ( $1 \div \sqrt{214.86}$ ) : velocity of gravitating force at Jupiter ( $1 \div 1051.298^2$ ), the units of velocity being taken, respectively, at Sun's surface and at the limit of equilibrium between ☉ & aggregation or dissociation ( $1.4232 + 1049.875$ ).

16. The same chief planets are also connected by the proportion,  $\sqrt{\text{modulus}}$  ( $688.3$ ) : light-producing wave at the mean-perihelion centre of gravity of Sun and Jupiter ( $\pi \times 1.0198$ ) : : Earth's mean radius vector : Sun's radius. The value of Earth's radius vector thus found is  $214.842$ , the value which is derived from the observations which I have thought the most accurate being  $214.86$ .

\* The "selenium eye" illustrates one of these harmonics. Professor W. G. Adams (Proc. Roy. Soc. Jan. 6, 1876; Phil. Mag. April 1876) says, "the change in the resistance of selenium is directly as the square root of the illuminating power." This is inversely as the velocity of nuclear rotation. The deflections in the dark (32) and in strong sunlight (470) give the ratio  $14.69$ , the ratio of emanating force at Sun to that at Earth's orbit being  $14.66$ .

17. Jupiter and Sun thus appear to be companion constituents of a binary star; and the point of primitive rupture should be sought at the secular-perihelion centre of gravity. Bodies falling towards that point, on approaching Sun, are subject to a force of about 1048 towards Sun, and 1049 towards the slowly moving common centre of gravity. There are therefore two nodal points, with the least resistance to motion nearly midway  $\left(\frac{1048}{2097}\right)$  between them. If Sun is gaseous, as Hunt, Faye, and others have supposed, there should hence arise linear oscillations of  $2 \times 2r$  synchronous with the circular oscillations of  $2\pi r$ . The corona may perhaps be due to such radial oscillations.

18. The 15th accordance gives for the mass of Sun  $\div$  Jupiter,  $1049.875 - 2 = 1047.875$ ; the 17th,  $1049.871 - 2 = 1047.874$ , Bessel's estimate being  $1047.879 \pm .235$ .

19. The discrepancy between the two astronomical estimates for the velocity of light seems to have arisen from ignorance of the intranodal oscillation. Delambre, from his discussion of more than 1000 eclipses of Jupiter's first satellite\*, estimated the time of light-passage from Sun to Earth at 493.198 seconds; Struve, from the phenomena of aberration, at 497.827 seconds. If the time of traversing  $212.86$  solar radii is 493.198, the time for  $214.86r$  should be 497.831, which differs from Struve's value by less than  $\frac{1}{1000}$  of 1 per cent.

20. If Earth was at the nebular nucleal *surface* when the Jovi-Saturnian ring was nebularly atmospheric, the *vis viva* of interior nucleal rotation varied as  $r$ , and the velocity of resulting planetary revolution as  $r^{\frac{1}{2}}$ . We thus obtain for the theoretical time of present solar rotation,

$$\sqrt{214.86} : 1 :: 365.2 \text{ days} : 24.912 \text{ days.}$$

The lowest estimate from observation is Spicer's, 24.624; the highest, that of Schwabe, 25.507.

21. The laws of central forces require that provision should be made for radial oscillations, tending towards the time-limit of isoradial circular oscillations ( $\sqrt{32}$  and  $\sqrt{8}$ ), for tangential velocities, varying inversely as the times and directly as the fourth root of central isoradial *tendencies*, for centres of oscillation in lines of force, and for oscillations between systematic and locally dominant centres. We have already seen (15, 17, 18) how closely the relative masses of Sun and Jupiter provide for the last requirement; if they provide also for the others, the centre of oscillation for Sun's possible atmosphere should be at

\* Stockwell, Proc. Amer. Assoc. vol. xx. pp. 76, 77.

$1047.875 - \sqrt{(32)^4} = 23.875$  solar radii. The corresponding height of possible atmosphere, or the height of equality between the velocities of rotation and revolution, is  $\frac{3}{2}$  of  $23.875 = 35.813 r$ . This would give, for the time of solar rotation,

$$365.256 \div (214.86 \div 35.813)^{\frac{3}{2}} = 24.856 \text{ days,}$$

differing less than  $\frac{1}{4}$  of 1 per cent. from the *vis-viva* estimate (20).

22. The ratio  $1 : 32^2$  is also simply connected with the mass and distance of Jupiter's companion planet Saturn, and so with the centre of planetary inertia. For  $1024 + 1025 + .536 = 2049.54$  (3, 17), Saturn's mean radius vector being  $2049.51$  solar radii;  $1024 + (35.13 \div \sqrt{4}) = 1080.625$  is the limit, of which Jupiter represents a centre of explosive oscillation, and the inertial moment at the limit gives the mass of Sun  $\div$  Saturn; for  $1080.625 \times (\frac{9}{5})^2 = 3501.2$ , Bessel's value being  $3501.6$ .

23. Among the many harmonies of planetary mass which manifest a dependence on nebular influences, the following are perhaps indicative of some of the earliest forms of activity:—

a. The masses of Jupiter and Earth are nearly proportionate to the square of their periodic times  $\times$  the velocities due to internuclear *vis viva*:

$$5.2028^3 \times 5.202.8^{\frac{1}{2}} = 321.2; 321.2 \times 1047.875 = 336201.$$

b. The influence of *spherical* gyration on Venus (11) seems to be further shown by its ratio to Earth, which is the *square* of the ratio of Uranus to Neptune ( $\sqrt{8} : \pi$ );  $\pi^2 \times 336201 \div 8 = 415289$ . Hill's estimate of the mass of Sun  $\div$  Venus is  $408134$ . If the internuclear *vis viva* of Jupiter were taken at secular perihelion, the resulting theoretical mass-denominators would be Earth  $326222$ , Venus  $402460$ .

24. The masses of the principal planets therefore seem to have been primitively determined by the following influences:— Neptune, by the proportion between the time of direct fall to the centre of planetary inertia, and the time of circular revolution; Uranus, by the time of describing the Sun's proportional part of a circle in the circular orbit; Saturn, by equality of nebular *vis viva* with Jupiter when the two centres of condensation were in opposite parts of the nebular belt and on opposite sides of the Sun; Jupiter, by the ratio of variability between incipient fall to a centre of linear atmospheric oscillation and circular revolution; Earth, by the combined action of *vis viva* and times of revolution; Venus, by the action, in a spherical mass, of the same forces as determined Uranus in a circular disk. The mass-denominators are:—

		Theoretical.	Observed.	Authority.
$\mathcal{A}$	$32^2 + 23.875$	1047.875	1047.879	Bessel.
$h$	$\left(\frac{9.539}{5.203}\right)^2 \times \mathcal{A}$	3522.3	3501.6	"
$\Psi$	$\sqrt{32} \times h$	19925.3	19700	Newcomb.
$\S$	$\pi \times \Psi \div \sqrt{8}$	22133.4	22600	"
$\oplus$	$5.203^3 \times 4.886^{\frac{1}{2}} \times \mathcal{A}$	326223	322800	"
$\ominus$	$\pi^2 \times \oplus \div 8$	402460	408134	Hill.

25. These masses, with Hansen and Olafsen's mass for Mars, and Encke's for Mercury, give the following ratios for the extra- and intraasteroidal groups :—

Theoretical.	Observed.	Theoretical.	Observed.
$\mathcal{A}$ 1.0000	1.0000	$\oplus$ 1.0000	1.0000
$h$ .2975	.2993	$\ominus$ .8106	.7909
$\Psi$ .0526	.0532	$\S$ .1019	.1009
$\S$ .0473	.0464	$\S$ .0670	.0663
1.3974	1.3989	1.9795	1.9581

Here is a further approximation, in the inner system, to the square of the outer ratio, accompanied by suggestive indications of the influence of the ratio between aggregating and dissociating velocities ( $1 : \sqrt{2}$ ), and of the ratio between the oscillatory and kinetic radii ( $1.4232 : 2 :: 1 : 1.405$ ). The outer is about  $220 \times$  the inner, or nearly as Earth's radius vector is to Sun's radius.

26. To these primitive influences others were subsequently added, depending upon mutual actions and reactions, some of which have already been pointed out, and others are obscurely intimated by harmonies which can hardly be regarded as accidental. To this later class the following may be added :—

a. Jupiter's mass is to Neptune's mass as  $\pi \times$  Neptune's radius vector is to Jupiter's radius vector ; Neptune's mass is to Earth's mass as  $\pi \times$  Jupiter's radius vector is to Earth's radius vector.

b. The mass of the intraasteroidal planets : Sun's mass :: square of Jupiter's secular aphelion : square of light-modulus.

c. The limit of possible solar atmosphere :  $6^2 \times$  Sun's radius :: Earth's polar : Earth's equatorial diameter ( $6 = 2 \times 3 =$  product of number of gravity nodal divisions by number of oscillatory nodal divisions in a linear pendulum : cf. Jupiter's mass =  $6\pi \times$  Neptune's mass).

27. Struve estimated Sun's proper motion at  $1.623 \times$  Earth's radius vector per annum, which is .258 times Earth's orbital motion, the motions being therefore in the ratio of their densities.



IV. *On some Problems connected with the Flow of Electricity in a Plane.* By OLIVER J. LODGE, B.Sc.

[Continued from vol. i. p. 389.]

*General form of the resistance-expression for two poles.*

§ 22. IT is convenient at this point to notice the general form of the resistance expression for two poles A and B in any bounded plate. It may always be written in the form

$$\frac{c}{\pi\kappa\delta}\log\left(\frac{AB}{\rho}\cdot Q\right), \quad \dots \dots \dots (\gamma)$$

where  $Q$  is a numerical quantity which depends on the general form of the boundaries of the plate, and on the position of the poles with respect to those boundaries; while  $c$  is a number which depends only on that part of the boundary which is infinitely near a pole. As long as the poles are not placed on the edge of the plate,  $c=1$  and is unaffected by any change in the boundary; but if either pole is placed on the edge, the angle on which it is placed determines  $c$ ,—the general law being that if A is on an angle  $\frac{\pi}{m}$ , and B on an angle  $\frac{\pi}{n}$ , then  $c=m+n$ . (When both poles are inside the plate, both angles equal  $2\pi$ , and therefore  $c=1$ , as stated above.)

It is unnecessary to prove this rule formally; but it is a consequence of the necessary condition that A and B must be of equal strength in the limited sheet; for this condition requires that if they are on angles  $\frac{2\pi}{2m}$  and  $\frac{2\pi}{2n}$  respectively, their strengths in the unlimited sheet must be in the ratio of  $2m:2n$ , and accordingly the quantity  $\frac{AB}{\rho}$  in (α) § 8 will occur to the  $2(m+n)$ th power.

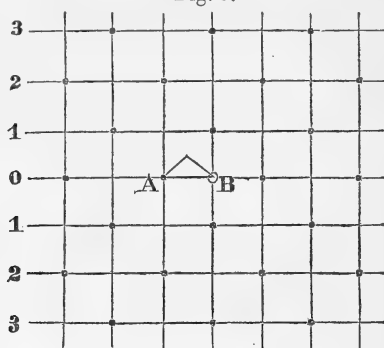
Hence, in the process of finding the resistance of any plane conductor containing two small electrodes, the determination of  $Q$  is the only difficulty. We have found it for the general polygon of two sides; let us proceed to find it for a few triangles.

*Resistance of an isosceles right-angled triangle. Poles on the equal angles.*

§ 23. This case was in reality the first figure with rectilinear boundaries which I attempted. The positions of the images of A are shown in fig. 5; they are on the corners of squares covering all the plane. The images of B will be similarly arranged, every source being surrounded by four sinks.

To determine the value of the product  $\left(\frac{AB}{\rho} \cdot Q\right)$  in the resistance-expression  $(\beta)$  (§ 8), take the images in rows, and find the product for each row separately (cf. § 9). Call the line A B,

Fig. 5.



Images of the pole A in an isosceles right-angled triangle.

fig. 5, row 0; the two rows one on either side of A B call rows 1; row 2 will be the row next beyond each of these; and so on, the images being evidently symmetrical with respect to the line A B. The whole product Q will then equal  $Q_0, Q_1^2, Q_2^2 \dots$ , where  $Q_x$  stands for the product of the  $\frac{A_x B}{A_x A}$ 's in one row  $x$ .

From the figure we see that

$$Q_0 = \frac{1 \cdot 3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \dots}{2 \cdot 2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \dots} = \frac{2}{\pi} = \frac{1}{\varpi(0)},$$

$$Q_1^2 = \frac{1^2}{1^2 + 1^2} \cdot \frac{2^2 + 1^2}{1^2 + 1^2} \cdot \frac{2^2 + 1^2}{3^2 + 1^2} \cdot \frac{4^2 + 1^2}{3^2 + 1^2} \cdot \frac{4^2 + 1^2}{5^2 + 1^2} \dots = 1^2 \cdot \varpi^2(1),$$

$$Q_2^2 = \frac{1^2 + 2^2}{2^2} \cdot \frac{1^2 + 2^2}{2^2 + 2^2} \cdot \frac{3^2 + 2^2}{2^2 + 2^2} \cdot \frac{3^2 + 2^2}{4^2 + 2^2} \dots = \frac{1}{2^2 \varpi^2(2)},$$

$$Q_3^2 = \frac{3^2}{1^2 + 3^2} \cdot \frac{2^2 + 3^2}{1^2 + 3^2} \cdot \frac{2^2 + 3^2}{3^2 + 3^2} \dots = 3^2 \varpi^2(3),$$

and in general

$$Q_x^{\pm 1} = x \varpi(x),$$

where the index is to be taken positive when  $x$  is odd, negative when  $x$  is even; and where  $\varpi(x)$  stands for the product

$$\frac{2^2 + x^2}{1^2 + x^2} \cdot \frac{4^2 + x^2}{3^2 + x^2} \cdot \frac{6^2 + x^2}{5^2 + x^2} \dots \frac{1}{\sqrt{(\infty^2 + x^2)}},$$

of which Wallis's expression is the particular case when  $x=0$ .

The entire product  $Q$  also shows an evident resemblance to Wallis's form, being

$$Q = \frac{2}{\pi} \left( \frac{1\varpi(1) \cdot 3\varpi(3) \dots}{2\varpi(2) \cdot 4\varpi(4) \dots} \right)^2.$$

Further than this I was unable to proceed; so I showed the products to Mr. J. W. L. Glaisher during the Bristol Meeting of the British Association; and he very kindly told me how to evaluate the product denoted above by  $\varpi(x)$ , and subsequently worked out the compound product  $Q$ . Taking the well-known trigonometrical identities

$$\sin \frac{\pi x}{2} = \frac{\pi x}{2} \left(1 - \frac{x^2}{2^2}\right) \left(1 - \frac{x^2}{4^2}\right) \left(1 - \frac{x^2}{6^2}\right) \dots,$$

$$\cos \frac{\pi x}{2} = \left(1 - \frac{x^2}{1^2}\right) \left(1 - \frac{x^2}{3^2}\right) \left(1 - \frac{x^2}{5^2}\right) \dots,$$

Mr. Glaisher divided one by the other after putting  $x\sqrt{-1}$  for  $x$ ; he thus obtained

$$\begin{aligned} \tan \frac{i\pi x}{2} &= \frac{i\pi x}{2} \cdot \frac{2^2 + x^2}{1^2 + x^2} \cdot \frac{4^2 + x^2}{3^2 + x^2} \dots \frac{1^2 \cdot 3^2 \cdot 5^2 \dots}{2^2 \cdot 4^2 \cdot 6^2 \dots} \\ &= \frac{i\pi x}{2} \cdot \varpi(x) \cdot \frac{2}{\pi}, \end{aligned}$$

whence

$$x\varpi(x) = \frac{1}{i} \tan \frac{i\pi x}{2} = \tanh \frac{\pi x}{2}^*.$$

This result gives us

$$Q = \frac{2}{\pi} \left( \frac{\tanh \frac{\pi}{2} \cdot \tanh \frac{3\pi}{2} \dots}{\tanh \frac{2\pi}{2} \cdot \tanh \frac{4\pi}{2} \dots} \right)^2.$$

The part inside the brackets may be written

$$\sqrt{\frac{\pi Q}{2}} = \frac{1 - e^{-\pi}}{1 + e^{-\pi}} \cdot \frac{1 - e^{-3\pi}}{1 + e^{-3\pi}} \dots \frac{1 + e^{-2\pi}}{1 - e^{-2\pi}} \cdot \frac{1 + e^{-4\pi}}{1 - e^{-4\pi}} \dots;$$

and this Mr. Glaisher saw at once was a special case of Jacobi's products; two of which (being those required in this paper) I write down here—

\* Mr. Glaisher also obtained an expression for the more general product  $\frac{2^n + x^n}{1^n + x^n} \cdot \frac{4^n + x^n}{3^n + x^n} \dots$ , which he has communicated to the Mathematical Society.

$$\frac{1-q}{1+q} \cdot \frac{1-q^3}{1+q^3} \cdot \frac{1-q^5}{1+q^5} \dots = k^{\frac{1}{4}}, \quad \dots \quad (\delta)$$

$$\frac{1-q^2}{1+q^2} \cdot \frac{1-q^4}{1+q^4} \cdot \frac{1-q^6}{1+q^6} \dots = \left( \frac{2\sqrt{k'K}}{\pi} \right)^{\frac{1}{2}}; \quad \dots \quad (\epsilon)$$

$q$  stands for  $e^{-\pi \frac{K'}{K}}$ ,  $K$  having its ordinary meaning of

$$\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{(1-k^2 \sin^2 \theta)}}.$$

Taking the modulus-angle  $45^\circ$  so that  $k=k'=\frac{1}{\sqrt{2}}$  and  $K=K'=K_0$  say, and then dividing  $(\delta)$  by  $(\epsilon)$ , there results the product required,

$$\sqrt{\frac{\pi Q}{2}} = \sqrt{\frac{\pi}{2K_0}}.$$

Hence  $Q = \frac{1}{K_0}$ , and the resistance of a triangle with each electrode on an angle of  $45^\circ$  is

$$R = \frac{8}{\pi \kappa \delta} \log \left( \frac{AB}{\rho} \cdot \frac{1}{K_0} \right). \quad \dots \quad (17)$$

The numerical value of  $K_0$  from Legendre's Tables is  
1.854 074 677 301.

§ 24. If one writes out (17) in two parts,

$$\frac{8}{\pi \kappa \delta} \log \frac{AB}{\rho} - \frac{8}{\pi \kappa \delta} \log \int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{(1-\frac{1}{2} \sin^2 \theta)}}, \quad \dots \quad (17)'$$

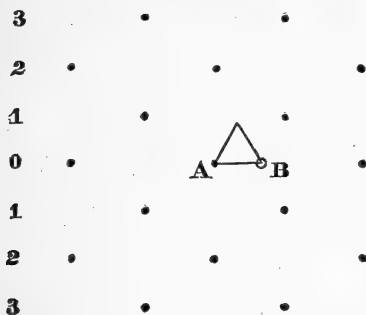
the first term is the resistance of a segment of a circle with base  $AB$  and with its arc touching the two sides of the triangle at  $A$  and  $B$ ; and the second term may be called the effect of the "coquadrant"\* by which the triangle exceeds the segment in area. But one must not imagine that this term expresses the resistance of this figure. The fact is that its resistance cannot be found by any consideration of  $(17)'$ , for reasons which shall be stated, § 30, footnote. It is worth while to notice the arrangement of poles and flow-lines which would produce this coquadrant figure in an unlimited sheet; but they will come better later.

\* I call it a *coquadrant* because it is the excess of a square over the quadrant whose radius is a side of the square: more generally the figure enclosed by any circular arc and two tangents might be called a *cosector*; it would be the excess of an isosceles cirquadrilateral over the inscribed sector, and it only differs from a sector in having the equal angles  $6^\circ$  instead of  $90^\circ$ .

Resistance of an equilateral triangle. Poles on two of the angles.

§ 25. The images of A for this case are shown in fig. 6,

Fig. 6.



Images of the pole A in an equilateral triangle.

being on the vertices of equilateral triangles  $\sqrt{3}$  times the linear dimensions of the original one. Take the images in rows as before (§ 23), calling the line A B row 0, and find the product  $Q_x$  for each row separately.

$$Q_0 = \frac{2 \cdot 4 \cdot 5 \cdot 7 \cdot 8 \cdot 10 \dots}{3 \cdot 3 \cdot 6 \cdot 6 \cdot 9 \cdot 9 \dots} = \frac{3\sqrt{3}}{2\pi} = \sqrt{0^2 \phi(0) \psi(0)},$$

$$Q_1^2 = \frac{1^2 + 3}{3^2 + 3} \cdot \frac{5^2 + 3}{3^2 + 3} \cdot \frac{7^2 + 3}{9^2 + 3} \cdot \frac{11^2 + 3}{9^2 + 3} \cdot \frac{13^2 + 3}{15^2 + 3} \dots = \phi(\sqrt{3}),$$

$$Q_2^2 = \frac{2^2 + 2^2 \cdot 3}{2^2 \cdot 3} \cdot \frac{4^2 + 2^2 \cdot 3}{6^2 + 2^2 \cdot 3} \cdot \frac{8^2 + 2^2 \cdot 3}{6^2 + 2^2 \cdot 3} \cdot \frac{10^2 + 2^2 \cdot 3}{12^2 + 2^2 \cdot 3} \dots = \psi(2\sqrt{3}),$$

$$Q_3^2 = \frac{1^2 + 3^2 \cdot 3}{3^2 + 3^2 \cdot 3} \cdot \frac{5^2 + 3^2 \cdot 3}{3^2 + 3^2 \cdot 3} \cdot \frac{7^2 + 3^2 \cdot 3}{9^2 + 3^2 \cdot 3} \dots = \phi(3\sqrt{3}),$$

. . . . . ;

and

$$Q = \frac{3\sqrt{3}}{2\pi} \phi(\sqrt{3}) \psi(2\sqrt{3}) \phi(3\sqrt{3}) \psi(4\sqrt{3}) \dots$$

Now

$$\begin{aligned} \phi(x) &= \frac{1^2 + x^2}{3^2 + x^2} \cdot \frac{5^2 + x^2}{3^2 + x^2} \cdot \frac{7^2 + x^2}{9^2 + x^2} \dots = \frac{1}{4} \cdot \frac{\cosh \frac{\pi x}{2}}{\cosh^3 \frac{\pi x}{6}} \\ &= \frac{1 + 3 \tanh^2 \frac{\pi x}{6}}{4} \end{aligned}$$

this result being evident if numerator and denominator are multiplied by

$$3^2 + x^2, 9^2 + x^2, 15^2 + x^2 \dots;$$

and similarly

$$\begin{aligned} \psi(x) &= \frac{2^2 + x^2}{x^2} \cdot \frac{4^2 + x^2}{6^2 + x^2} \cdot \frac{8^2 + x^2}{6^2 + x^2} \dots = \frac{1}{4} \cdot \frac{\sinh \frac{\pi x}{2}}{\sinh^3 \frac{\pi x}{6}} \\ &= \frac{1 + 3 \coth^2 \frac{\pi x}{6}}{4}. \end{aligned}$$

Hence

$$\begin{aligned} Q &= \frac{3\sqrt{3}}{2\pi} \cdot \frac{1 + 3 \tanh^2 \frac{\pi}{2\sqrt{3}}}{4} \cdot \frac{1 + 3 \tanh^2 \frac{3\pi}{2\sqrt{3}}}{4} \cdot \frac{1 + 3 \tanh^2 \frac{5\pi}{2\sqrt{3}}}{4} \dots \\ &\quad \times \frac{1 + 3 \coth^2 \frac{2\pi}{2\sqrt{3}}}{4} \cdot \frac{1 + 3 \coth^2 \frac{4\pi}{2\sqrt{3}}}{4} \cdot \frac{1 + 3 \coth^2 \frac{6\pi}{2\sqrt{3}}}{4} \dots \end{aligned}$$

This product I sent to Mr. Glaisher; and he returned it reduced to a product of two theta-functions. He proved the two following identities:—

$$\begin{aligned} P_1 &= \frac{1 + m \tanh^2 \theta}{1 + m} \cdot \frac{1 + m \tanh^2 3\theta}{1 + m} \cdot \frac{1 + m \tanh^2 5\theta}{1 + m} \dots \\ &= \sqrt{\frac{\pi}{2K}} \Theta\left(\frac{2Kx}{\pi}\right) \end{aligned}$$

and

$$\begin{aligned} P_2 &= \frac{1 + m \coth^2 2\theta}{1 + m} \cdot \frac{1 + m \coth^2 4\theta}{1 + m} \cdot \frac{1 + m \coth^2 6\theta}{1 + m} \dots \\ &= \frac{1}{\cos x} \sqrt{\frac{\pi^3}{(2K)^3 k k'}} \cdot H\left(\frac{2Kx}{\pi} + K\right), \end{aligned}$$

where

$$\cos 2x = \frac{m-1}{m+1} \text{ and } 2\theta = \pi \frac{K'}{K}.$$

In our present case

$$m = 3 \text{ and } \theta = \frac{\pi}{2\sqrt{3}};$$

so

$$K = \sqrt{3}K', \quad k = \sin 75^\circ, \text{ and } x = \frac{\pi}{6}.$$

Hence

$$P_1 = \sqrt{\frac{\pi}{2K}} \Theta\left(\frac{1}{3}K\right), \quad \frac{\sqrt{3}}{2} P_2 = \sqrt{\frac{\pi^3}{8K^3 k k'}} H\left(\frac{2}{3}K\right),$$

and

$$Q = \frac{3\sqrt{3}}{2\pi} P_1 P_2 = \frac{3\pi}{4K^2 \sqrt{k'}} \Theta(30^\circ) \Theta_1(60^\circ) \pmod{\text{angle } 75^\circ},$$

writing  $\Theta_1(x)$  for  $\frac{1}{\sqrt{k}} H(x)$ .

From the British-Association Tables

$$\log \frac{1}{K^2 \sqrt{k'}} = \bar{1} \cdot 4091498986,$$

$$\log \Theta(30^\circ) = \bar{1} \cdot 9223413942,$$

$$\log \Theta_1(60^\circ) = \cdot 0491498189;$$

so

$$\log_{10} Q = \bar{1} \cdot 7528522478,$$

or

$$Q = \frac{1}{1 \cdot 76664} \text{ about,}$$

and the resistance of the triangle is

$$R = \frac{6}{\pi \kappa \delta} \log \left( \frac{AB}{\rho} \cdot Q \right). \quad . \quad . \quad . \quad . \quad . \quad (18)$$

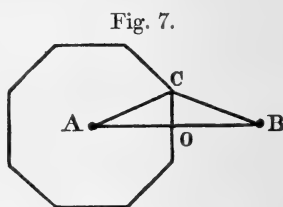
*Manner in which the image-method fails for an angle of  $120^\circ$ .*

§ 26. We found in § 5 that the only isosceles triangles which could be treated by the method of images were those with the equal angles either  $0^\circ$ ,  $45^\circ$ ,  $60^\circ$ ,  $90^\circ$ , or  $180^\circ$ ; and of these all except the first have been already done in the course of this paper (§§ 23, 25, 19, 10), and the first shall be considered in the next section. But suppose we attempted the triangle with equal angles  $30^\circ$ , which is apparently a very simple case; the images of the source A (on one of the equal angles) would be readily placed on the corners of regular hexagons; but they would be symmetrically situated with respect to the middle line of the triangle, and hence when the images of the sink B came to be placed they would coincide with those of A and would blot them all out of the sheet. Or if we overlook this and apply the expression ( $\beta$ ) to the images of A, then, because of their symmetry with respect to the triangle, the product Q (§§ 8 & 22) will equal 1; which can be proved to be wrong (§ 27). This failure, caused by the presence of the angle  $120^\circ$ , is one that cannot, even apparently, be got over by silvering both

sides of the boundaries (§ 4); for, in order to arrange poles in an unlimited sheet so as to be symmetrical with respect to all three sides of the triangle, it is necessary that one and the same point shall be at the same time both a source and a sink. Dr. Henrici suggests that this is possible if the plane consists of two leaves, and recommends in general, whenever both sides of a boundary have to be silvered and some of the images are *real* (that is, occur in the given plate itself), that these real images be put in another leaf of the plane. It seems just possible that some such contrivance might enable the image-method to be applied to polygons whose angles are not integral submultiples of  $\pi$ .

*Empirical formula for the resistance of a general isosceles triangle. Poles on the equal angles.*

§ 27. Consider a regular polygon of  $n$  sides, with one electrode (radius  $\rho$ ), A, fig. 7, at its centre, and with its entire periphery maintained at one potential. The resistance  $R_n$  of such a polygon to radial flow is evidently something between that of its inscribed and that of its circumscribing circle; in other words,



$$\frac{1}{2\pi\kappa\delta} \log \frac{AO}{\rho} < R_n < \frac{1}{2\pi\kappa\delta} \log \frac{AC}{\rho}.$$

Now the resistance  $R$  of the isosceles triangle  $ABC$  (fig. 7) equals twice the resistance of the triangle  $AOC$ , which again equals  $2n$  times the resistance of the polygon. Hence, writing

$$AC = AO \sec \angle CAO = \frac{1}{2} AB \sec \frac{\pi}{n},$$

$$\frac{2n}{\pi\kappa\delta} \log \frac{AB}{2\rho} < R < \frac{2n}{\pi\kappa\delta} \log \left( \frac{AB}{2\rho} \sec \frac{\pi}{n} \right);$$

or, calling the angle  $\angle CAO$   $\theta$ , we may write the resistance of any isosceles triangle with equal angles  $\theta$ ,

$$R = \frac{2}{\theta\kappa\delta} \log \left( \frac{AB}{\rho} \frac{1}{f(\theta)} \right), \quad \dots \dots (19)$$

where  $f(\theta)$  is something between  $2 \cos \theta$  and  $2$ . Hence the limit of  $Q$  when the angles of the triangle vanish is  $\frac{1}{2}$ . Moreover when the angles are  $30^\circ$ ,  $f(\theta)$  must lie between  $2$  and  $\sqrt{3}$ ; it cannot, therefore, be  $1$  as the product of images (§ 26) would lead us to believe.

Looking at the few values of  $f(\theta)$  which are known, one



may notice how nearly they divide the interval allowed to them,  $2-2\cos\theta$ , in the ratio 1:3. The quantity which actually does this is  $\frac{3+\cos\theta}{2}$ ; and this quantity agrees with the known values of  $f(\theta)$  to an extent shown in the Table.

$\theta$ .	$f(\theta)$ .	$\frac{3+\cos\theta}{2}$ .
0	2.0	2.0
45	1.854	1.853
60	1.766	1.75
90	1.57	1.50
180	1.0	1.0

The maximum divergence appears to occur about when  $\theta=90^\circ$ .

The empirical formula, then,

$$\frac{2}{\theta\kappa\delta}\log\left(\frac{AB}{\rho}\cdot\frac{2}{3+\cos\theta}\right)\cdot\cdot\cdot\cdot\cdot\quad(19)$$

gives the resistance of any isosceles triangle with poles on the equal angles  $\theta$  with sufficient accuracy for a practical purpose; and it becomes very accurate indeed when the equal angles are small. The value of  $Q$  indicated by it for the case  $\theta=30^\circ$  is

$\frac{1}{1.9330}$ ; and I venture to think that the real value will be found to differ from this only in the last place of decimals.

The resistance to the flow of electricity from a central electrode of radius  $\rho$  to the periphery of a regular polygon of  $n$  sides is, to the same degree of approximation as the above,

$$\frac{1}{2\pi\kappa\delta}\log\left(\frac{r}{\rho}\cdot\frac{4}{3+\cos\frac{\pi}{n}}\right),\cdot\cdot\cdot\cdot\cdot\quad(20)$$

$r$  being the radius of the inscribed circle.

§ 28. And here I would remark that the thickness ( $\delta$ ) of the plate in which the flow takes place has never been limited in any way; so the plate may extend itself into a cylinder of any length, provided the point poles extend themselves into lines at the same time. Also, though we are using the language of dynamic electricity only, yet the resistance obtained applies equally well to heat if we substitute hot and cold bodies for sources and sinks, temperature for potential, and isothermals for equipotential lines. Further, by reason of the analogy between conductivity and inductive capacity, we obtain at the

same time the inductive capacity of a dielectric medium exposed to conductors at different potentials which correspond to the poles in the dynamic problem.

*Resistance of some squares and right-angled triangles.*

§ 29. The forms assumed by the resistance-expressions (§ 8) for the case of a square plate with the poles in certain definite positions may be recorded, together with the products which lead to them. The resistance seems always to be of the form

$$R = \frac{m+n}{\pi\kappa\delta} \log \left( \frac{AB}{\rho} \cdot \frac{2^\alpha}{K_0} \right), \quad \dots \dots (21)$$

where the poles A and B are on angles  $\frac{\pi}{m}$  and  $\frac{\pi}{n}$  respectively, and where  $\alpha$  takes different values in different cases, and is the only thing which varies.  $K_0$  still stands for the first complete elliptic integral with modulus  $\frac{1}{\sqrt{2}}$ .

For the case represented in fig. 8 the value of  $\alpha$  is zero, because the resistance of such a square is just half that of the right-angled triangle (17) § 23.

When the poles are as in fig. 9,  $\alpha = \frac{1}{4}$ ; the product in ( $\beta$ ) which leads to the result consists of terms  $\coth^2 \frac{\pi x}{2}$ , where  $x$  takes the values 2, 4, 6, ...

The case of fig. 10 gives us  $\alpha = \frac{1}{2}$ , the product being  $\Pi \left( \coth^2 \frac{\pi x}{2} \right)$ , with  $x$  successively 1, 2, 3, 4, &c.

The case of fig. 11, being unsymmetrical in sources and sinks, requires the complete expression ( $\alpha$ ) § 8; and, because the pole at the corner of the square has to be four times as strong as the other in the infinite sheet, the two factors inside the brackets of ( $\alpha$ ) occur to different powers; in other words, calling the factors  $P_1$  and  $P_2$ , we shall have

$$Q^5 = P_1 P_2^4.$$

Now  $P_1$ , the product referring to the images of the pole at the centre, is the old one whose value is  $\frac{1}{K_0}$  (§ 23). But the pro-

Fig. 8.

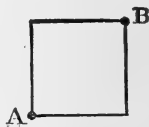


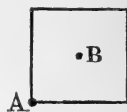
Fig. 9.



Fig. 10.



Fig. 11.



duct for the images of the corner pole will be found to be

$$P_2 = \frac{2\sqrt{2}}{\pi} \Pi \left( \coth \frac{\pi x}{2} \coth \frac{\pi x}{4} \right) \Pi \left( \frac{\coth \frac{\pi x'}{2}}{\coth \frac{\pi x'}{4}} \right),$$

where  $x$  is to take the successive values 4, 8, 12, ..., and  $x'$  the values 2, 6, 10, ...

Writing this product  $P_2$  in the form

$$\frac{2\sqrt{2}}{\pi} \cdot \frac{\tanh \frac{\pi}{2} \cdot \tanh \frac{3\pi}{2} \dots}{\tanh \frac{2\pi}{2} \cdot \tanh \frac{4\pi}{2} \dots} \cdot \coth \pi \cdot \coth 2\pi \cdot \coth 3\pi \dots,$$

it is brought into connexion with  $(\delta)$  and  $(\epsilon)$  (§ 23); and its value is thus found to be

$$P_2 = \frac{2\sqrt{2}}{\pi} \sqrt{\frac{\pi}{2K_0}} \sqrt{\frac{\pi}{2^{\frac{3}{2}}K_0}} = \frac{2^{\frac{5}{8}}}{K_0}.$$

Hence

$$Q^5 = \frac{2^{\frac{5}{8}}}{K_0}, \text{ or } Q = \frac{\sqrt{2}}{K_0}.$$

In other words, the resistance of a square plate with the poles arranged as in fig. 11 is given by (21), if for  $m+n$  we read its value  $\frac{5}{2}$ , and consider  $\alpha$  equal to  $\frac{1}{2}$ .

One more distinct case may be mentioned, namely the right-angled triangle shown in fig. 12. This also requires two factors raised to different powers; and the result is the expression (21) with  $\alpha = \frac{1}{6}$ .

Fig. 12.



[To be continued.]

V. *Some Remarks on the Finite Integration of Linear Partial Differential Equations with constant Coefficients.* By the Rev. S. EARNSHAW, M.A.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IN your Magazine for June 1849 you kindly printed a short paper of mine "On the Transformation of Linear Partial Differential Equations with constant Coefficients to Fundamental Forms," in which I promised to make "in a future communication a few remarks on the finite integration" of equations of the second order with two or three independent

variables. With your permission I will now fulfil that promise.

In the communication referred to it was shown that these two classes of equations can by a change of variables be reduced to the following fundamental forms :—

$$(1) \quad \frac{d^2u}{dx dy} = au; \quad (2) \quad \frac{d^2u}{dx^2} = \frac{du}{dy}; \quad (3) \quad \frac{d^2u}{dx dy} = \frac{d^2u}{dz^2};$$

$$(4) \quad \frac{d^2u}{dx dy} = \frac{du}{dz}; \quad (5) \quad \frac{d^2u}{dx dy} = \frac{d^2u}{dz^2} + a \frac{du}{dz};$$

so that if we can succeed in integrating these forms, then we may consider the general linear partial differential equations of the second order, with two or three variables and with constant coefficients, to have been integrated. When certain relations among the coefficients exist the finite integration is easy; but in other cases the difficulty of finite integration was never overcome. I propose to point out the cause of the difficulty which occurs.

1. If  $U$  be an integral in finite terms, then are also  $\frac{dU}{dx}, \frac{dU}{dy}$ ; and if  $U$  be differentiated any number of times with regard to the independent variables, the results will be integrals also. Consequently from any one integral we can deduce an unlimited number of other integrals; and all these must be included with  $U$  in the general integral; and each must stand therein multiplied by a separate and independent arbitrary constant. And if so, how is their sum to be gathered into a finite form? Surely the arbitrary constants will be an insuperable barrier to finiteness of expression.

2. Again, if  $U$  contain in the body of it an arbitrary constant  $c$ , then will  $\frac{dU}{dc}, \frac{d^2U}{dc^2}, \frac{d^3U}{dc^3}, \dots$  be cointegrals with  $U$ , and their sum, each multiplied with an arbitrary constant, must enter into the general integral; so that, as before, we have what appears to be an insuperable barrier to finiteness of expression.

3. If  $U$  be expanded in a series in powers of  $c$ , each coefficient of its powers will be an integral, and we have again the same difficulty repeated. Thus there are three impediments, any one of which seems to be an insuperable barrier to finiteness of expression.

An example will make this clear. Let us take the equation  $\frac{d^2u}{dx^2} = \frac{du}{dy}$ . A known integral of this equation is

$$F(x, y) = A e^{ix + c^2 y}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If this be differentiated any number of times with regard to



VI. *A Contribution to the History of the Old Sulphur Well, Harrogate.* By T. E. THORPE, *Ph.D., F.R.S., Professor of Chemistry in the Yorkshire College of Science, Leeds*.\*

EDMUND DEAN, "Doctor in Physick, Oxon., dwelling in York," first drew public attention to the virtues of this famous spring, in his *Spandarine Anglica*, published in 1626.

"The waters of this sulphureous fountain," he says, "are very cold, and have no manifest heat, because their mines and veins of brimstone are not kindled under the earth, being hindered from the mixture of salt therewith." We are further told that although such as drank the water verily believed there was gunpowder in it, there could be little doubt of its efficacy against "reef and fellow," "morphew, tettars and the like." Its attributes are more fully set forth by Dr. Michael Stanhope in his 'Cures without Care, being a Summons to all such as find little or no keep by the use of Physick, to repair to the Northern Spaw, &c.,' published in 1632; and since that time the merits of "its most foetidly salutary vapours" (to use Pennant's phrase) have been repeatedly extolled. Although it came into use some time after the chalybeate springs in the neighbourhood, it would seem that when trials of its virtues came to be made, the well speedily acquired the pre-eminence which it now enjoys. Dr. Short, so far back as 1734, described the "stone-basin enclosed in a small neat building of stone and lime, about a yard square on the inside and nearly two yards high, covered over with two thick smooth flagstones," which still remains under the present pump-room.

The strongly marked character of the water, its regular flow, and the apparent uniformity of its composition, together with its undoubted therapeutic action, must have made it an object of curiosity even in very early times; and doubtless many trials were made to gain an insight into its nature. The first attempt at its quantitative analysis, of which any record has been preserved, was made by Dr. Higgins in 1780; he thus stated his results:—

	oz.	d.	grs.
"A Winchester gallon of Harrogate			
water contains of calcareous earth }	0	1	12 $\frac{1}{4}$
saturated with acidulous gas . . }			
Marine salt of magnesia . . . . .	0	4	23 $\frac{1}{4}$
Sea-salt . . . . .	1	7	12 $\frac{1}{2}$
	1	14	0

\* Read before the Priestley Club, Leeds. Communicated by the Author.

“It moreover contains four measures of acidulous gas beyond the quantity retained by the calcareous earth in the heat of boiling water, and thirty-five ounces of foetid inflammable gas, such as may be extricated from calcareous liver of sulphur by vitriolic acid.”

The quantity of solid matter above given is equal to 831 grains in the imperial gallon; it is probably underestimated. A more accurate analysis was made in 1783 by Dr. Joshua Walker; a full account of his method of operating and of his results is published in his treatise on the spring\*. He found that it contained 15 drachms of common salt and 26 grains of calcareous earth in the wine gallon, amounting in all to 1111·2 grains in the imperial gallon—no very great difference from its total solid contents at the present time. If by calcareous earth is meant the carbonates of lime and magnesia precipitated on evaporation, the number agrees fairly well with the present quantity of these bodies contained in the water. Among the other constituents are mentioned “hepar sulphuris,” “a small quantity of selenites,” and “sulphurous vapour in very large proportion.” Dr. Garnett, who examined the spring in 1791, also mentions sulphates to the extent of 13·1 grains of magnesium sulphate to the imperial gallon. If we may credit these statements, it is certain that the spring must have experienced at least one remarkable change during its history; for not a trace of sulphuric acid can now be detected. In Dr. Hofmann’s analysis made in 1853, calcium sulphate is mentioned to the amount of two tenths of a grain; but it is not at all improbable that the minute quantity of sulphuric acid, equal to about a milligramme in the litre, corresponding to this amount of the lime-salt, might have been formed by the oxidation of sulphuretted hydrogen subsequent to the time of collection. No other analyst, either before or since, makes mention of sulphates. Mr. West, who made a careful examination of the water in 1823, speaks positively as to their absence: “with salts of barytes [he employed the chloride, nitrate, and acetate] prepared by other chemists, as well as with my own, not the slightest cloud was produced”†. It is doubtful, indeed, if sulphates have been present in the water at any period of its existence. The analysis given by Dr. Hunter in his treatise on the Harrogate waters (published in 1830) was evidently made with care; and some of the numbers, especially those for the chlorine and soda and total

\* An Essay on the Waters of Harrogate and Thorp-arch. By Joshua Walker, M.D., Physician to the Leeds Infirmary, 1784.

† Quart. Journ. vol. xv. p. 82.

solid matter, agree closely with the determinations of later analysts\*. The total volume of the dissolved gases in the imperial gallon was estimated by Dr. Hunter at 34 cubic inches, and by Mr. West at 36·4 cubic inches.

The true chemical history of the spring may, however, be said to date from Dr. Hofmann's analysis, made nearly a quarter of a century ago†. His examination first made known with certainty the proper nature and proportion of the constituents of the water; it is therefore only since 1853 that we can determine with accuracy the character and extent of the changes which have taken place. The existence of barium and strontium in the water was first stated by Mr. Hayton Davis in 1866‡; he has since made a number of observations, extending over the year 1872, to determine the effect of the seasons on the spring§. An analysis was also made by the late Dr. Muspratt in 1867||. In the main his results agree closely with those of Dr. Hofmann.

The water, the analysis of which forms the main subject of the present communication, was collected in the early part of August 1875. When drawn from the well it was perfectly clear and colourless, smelled strongly of sulphuretted hydrogen, and had a decided alkaline reaction. When shaken it formed small pearly bubbles, and in a few hours became turbid from the separation of sulphur. After standing for some time it again became clear and acquired a faint yellow tinge; this change, which occurs more rapidly on warming with free exposure to air, is due to the formation of an alkaline bisulphide. The rate of the flow of the spring was estimated at 30 gallons in the hour. The temperature of the water at the time of collection was 10° C. (50° F.); in April 1876 it was 8° C. (46°·5 F.). Mr. Davis has already noticed similar variations; according to his observations its mean temperature in 1872 was 9°·4 C. (48°·9 F.). Dr. Hofmann in 1853 observed 9° C. (48°·2 F.). Dr. Walker in 1783 also observed 48° F.

The specific gravity of the water at 16°·8 C., compared with distilled water at the same temperature, was 1011·04; this number agrees closely with the observations of Dr. Hofmann and Mr. Davis.

The following substances were found to be present in deter-

\* Treatise on the Mineral Waters of Harrogate, &c. By Adam Hunter, M.D. (1830).

† "Chemical Analysis of the Medicinal Waters of Harrogate," Journ. Chem. Soc. vol. vii. p. 161.

‡ Chem. News, 1866.

§ Journ. Chem. Soc. 1873.

|| Chem. News, 1867.



minable quantities :—

Potash.	Chlorine.
Soda.	Bromine.
Ammonia.	Iodine.
Lithia.	Carbonic acid.
Baryta.	Sulphur.
Lime.	Silicic acid.
Magnesia.	

In addition were minute traces of strontia, fluorine, phosphoric acid, alumina, and organic matter. A special examination, made by means of the spectroscope, of the alkaline chlorides obtained from the chloroplatinates from the residue of 10 litres of the water, failed to reveal the presence of thallium, caesium, or rubidium.

In the quantitative analysis at least two determinations of each principal constituent were made. The results are as follows :—

	Parts per 1000.
1. Fixed constituents dried at 170°–190°	14·7940
2. Chlorine . . . . .	8·6724
3. Bromine . . . . .	0·02805
4. Iodine . . . . .	0·00146
5. Carbonic acid (total) . . . . .	0·5003
6. Lime and baryta together . . . . .	0·6154
7. Magnesia (total) . . . . .	0·3368
8. Lime and baryta in boiled water . . . . .	0·3795
9. Lime precipitated on boiling :—	
Total lime and baryta . . . . .	0·6154
Lime and baryta retained in solution on boiling . . . . .	0·3795
Lime precipitated on boiling . . . . .	0·2359
Equal to calcium carbonate . . . . .	0·4206
10. Lime retained in solution after boiling :—	
Lime and baryta . . . . .	0·3795
Deduct baryta . . . . .	0·0683
Lime . . . . .	0·3112
11. Baryta . . . . .	0·0683
12. Lithium chloride . . . . .	0·01064
13. Potassium chloride . . . . .	0·13553
14. Sodium chloride (total) . . . . .	12·7034
15. Ammonium chloride . . . . .	0·01457
16. Silica . . . . .	0·0099

17. *Determination of total Sulphur.*—A known quantity of the water was mixed with bromine immediately after being taken from the spring. After standing the barium sulphate formed was filtered off and weighed.

Water taken.	BaSO <sub>4</sub> obtained.	S in 1000 grams.
1028·69 grams.	0·1078 gram.	0·0144
1037·05    ,,	0·1070    ,,	0·0142
		Mean ... 0·0143

Barium chloride was added to the filtrates.

Water taken.	BaSO <sub>4</sub> obtained.	S in 1000 grams.
1028·69 grams.	0·5834 gram.	0·0780
1037·05    ,,	0·5884    ,,	0·0780
		Mean ... 0·0780

Total sulphur = 0·0923

18. *Determination of Sulphur oxidizable by Iodine solution.*—These determinations were made at the well in the usual manner, with standard iodine solution and starch (1 cub. centim. of the iodine solution contained 0·00118 gram I).

Water taken.	Iodine solution needed.	
50·50 grams.	31·5 cub. centims.	
50·50    ,,	31·4    ,,	
50·50    ,,	31·1    ,,	In 1000 grams.
50·50    ,,	31·5    ,,	Mean 31·4 cub. cent. = 0·0923 S.

It is evident, from the absolute agreement between the results of the two independent methods of determining the sulphur, that no thiosulphate can be present.

19. *Determination of the Sulphur as Hydrogen Sulphide, and as Alkaline Sulphide.*—A considerable amount of discussion has occurred respecting the manner in which the sulphur, perhaps the chief remedial agent in the water, is contained in it. It would seem to be certain from the above determinations that it exists entirely as a sulphide, either as hydrogen or alkaline sulphide, or more probably in both forms. Dr. Bennett, in his 'Treatise on the Sulphureous Springs of Harrogate,' published in 1843, states that no free sulphuretted hydrogen is present in this water, but that all the sulphur exists in combination with calcium and sodium; and in all the analyses which have been made subsequent to that time the sulphur has been calculated to sodium sulphide or sulphydrate. Dr. Bennett based his conclusions on the observations that no arsenic sulphide was precipitated on the addition of a solution of arsenious oxide to the water. This test, however, is fallacious, inasmuch as any arsenic sulphide which might be formed

would be retained in solution by the alkaline sulphide which Dr. Bennett inferred was alone present. It is quite certain that a portion of the sulphur in the water exists as free sulphuretted hydrogen.

In order to determine the ratio between the dissolved gas and the alkaline sulphide present, I followed the procedure adopted by Simmler in his analysis of the Stachelberg sulphur-water, of expelling the hydrogen sulphide by a current of an indifferent gas, such as hydrogen, and passing the escaping gas through an ammoniacal solution of silver nitrate, converting the precipitated sulphide into chloride, and determining the amount of sulphur from the weight of the latter compound. The operation (which extended over about twenty hours) was conducted immediately after the collection of the water, and before it had suffered the slightest exposure to air. Two experiments were carried on simultaneously. The results are as follows :—

Water taken.	AgCl obtained.	Dissolved $H_2S$ in 1000 grams.
1363·26 grams.	0·6271 gram.	0·0544
1355·75 „	0·5962 „	0·0521
		Mean ... 0·0533

*Determination of alkaline sulphide.*

Total sulphur . . .	=·0923
Equal to $H_2S$ . . . .	=·0980
Deduct free $H_2S$ . . . .	<u>·0533</u>
Combined $H_2S$ . . . .	=·0447
= Na HS ·07369.	

Search was made for carbon oxysulphide, which, according to Than, appears to exist in certain Hungarian sulphur waters. The method of testing was based upon the fact that when carbon oxysulphide is led into solution of ammonia, ammonium oxysulphocarbamate,  $NH_2COSNH_4$ , is formed, and that when the solution of this salt is heated to  $100^\circ$  in sealed tubes it loses the elements of water and is converted into ammonium sulphocyanate, the existence of which is readily demonstrated by the blood-red coloration it affords with ferric chloride. The gases evolved by boiling 2 litres of the recently collected water were led into dilute ammonia; and the ammoniacal solution, after concentration, was heated as above described. The residue left on evaporation gave not the slightest indication of the presence of a sulphocyanate.

The results of the analysis, arranged in the conventional mode of representing the state of combination of the various acids and bases, are as follows :—

	In 1000 grams.	In 1 gallon.
	grams.	grains.
Barium chloride . . . .	0·09277	6·566
Strontium chloride . . .	traces	traces
Calcium chloride . . . .	0·61649	43·635
Calcium fluoride . . . .	traces	traces
Calcium phosphate . . .	traces	traces
Magnesium chloride . . .	0·68212	48·281
Potassium chloride . . .	0·13553	9·592
Lithium chloride . . . .	0·01064	0·753
Ammonium chloride . . .	0·01457	1·031
Sodium chloride . . . .	12·62657	893·670
Sodium sulphhydrate . . .	0·07369	5·215
Magnesium bromide . . .	0·03226	2·283
Magnesium iodide . . . .	0·00160	0·113
Calcium carbonate . . . .	0·42061	29·768
Magnesium carbonate . .	0·08410	5·953
Silica . . . . .	0·00991	0·701
Alumina and organic matter	traces	traces
	14·80086	1047·561

Amount of soluble matter obtained by direct evaporation and dried at  $170^{\circ}$ – $190^{\circ}$ , 14·7940 grams in 1000 grams.

The amount of the solid matter obtained by direct evaporation is slightly lower than that calculated from the several results of the analytical operations. This, indeed, was to be expected from the character of certain of the constituents. The magnesium salts tend to become basic on drying; and the ammonium salt and the alkaline sulphhydrate would be decomposed.

It has not been thought necessary to ascertain the amount and nature of the gases expelled from the water by boiling. Such determinations have in great measure lost their significance since Dr. Jacobsen and Mr. Buchanan have shown that the saline matter in the water interferes greatly with the complete expulsion of the dissolved gases. The amounts, however, of free carbon dioxide and hydrogen sulphide (the most important of the gaseous constituents) are readily deduced from the analytical results. They are as follows:—

Vol. of dissolved gases at $10^{\circ}$ and 760 millims.	In 1000 grams.	In 1 gallon.
Sulphuretted hydrogen	36·28 cub. cent.	10·16 cub. in.
Carbon dioxide . . .	143·17     ,,	40·10     ,,

Almost exactly one half of the total amount of carbon dioxide in the water is in a state of combination, the other half being free or existing as the so-called acid carbonates. The volume of the free gases thus formed is considerably larger than is

obtained by the method of ebullition, although the ratios of the amounts of the gases agree closely with the observations of Hofmann and Muspratt (see Table), which seems to afford proof of the retentive action exerted by the dissolved saline matter.

The following Table exhibits the results in grains per gallon of the principal analyses which have been made of the water of the Old Sulphur Well. It has been arranged to show the various constituents independently of the views entertained by the analysts as to the particular manner of their combination in the water. We are thus enabled to trace, with a certain degree of probability, the nature of the changes which the spring has experienced from time to time.

Analyst .....	Walker.	West.	Hunter.	Hofmann	Muspratt	Davis.	Thorpe.
Date .....	1873.	1823.	1830.	1853.	1867.	1872.	1875.
Temp .....	48° F.	...	...	48° 2 F.	...	48° 9 F.	48° 2 F.
Sp. gr.....	...	1013.24	...	1011.13	...	1011.16	1011.04
Lime .....	...	39.8	43.5	48.243	46.233	...	38.697
Magnesia .....	...	14.7	18.0	23.446	27.392	...	23.839
Baryta .....	...	...	...	...	3.68	...	4.833
Lithia .....	...	...	...	...	trace	...	0.266
Potash .....	...	...	...	33.869	44.165	...	6.063
Ammonia .....	...	...	...	...	...	...	0.328
Soda .....	...	484.0	471.0	474.054	470.635	...	477.022
Chlorine.....	...	623.9	608.0	650.384	654.908	615.62	613.770
Bromine.....	...	...	...	trace	trace	...	1.985
Iodine .....	...	...	...	trace	trace	...	0.103
Sulphur .....	...	...	...	6.353	6.737	6.412	6.532
Carbon dioxide	...	...	...	...	...	...	35.404
Silica .....	...	...	...	0.241	...	...	0.703
Sulphuric acid.	...	absent	absent	0.101	absent	...	absent
Residue on } evaporation }	1111.2	1024.8	1016.0	1095.919	1108.781	1046.56	1047.013
Free H <sub>2</sub> S .....	...	...	...	5.31	7.01	...	10.16
„ CO <sub>2</sub> .....	...	...	...	22.03	25.55	...	40.10
Total in cub. in.	...	36.4	34.0	36.09	...	...	...

Although it is undoubtedly clear that the general composition of the water has experienced no very material alteration during the past 100 years, it would seem to be equally certain, even after making every allowance for experimental error and imperfection in analytical methods, that certain well-marked changes have occurred during that time. It would appear that the amount of soluble matter in the water suffered a gradual decrease from 1783 to 1830 ; afterwards it increased until 1867, since which time it has again become less. The close agreement between the numbers given by Mr. Davis and those afforded by the analysis of the water collected in August last, clearly shows that it has experienced very little change during

the last three years. As compared with the composition in 1853 and 1867, the most striking difference is exhibited in the quantity of potash, which is less than one seventh of the amount observed in the latter year. The amount of lime is also considerably less ; and the diminution in both constituents is accompanied by a corresponding decrease in the amount of chlorine. The amount of barium salt in the water is unusually large, and appears to be increasing. So far as is known, no mineral springs in this country or on the Continent contain so large a proportion of this substance as the Harrogate waters. The quantity, indeed, in the Old Sulphur Well is as large as the entire amount of soluble matter contained in many of the waters used for domestic supply in our towns. So potent an agent present in such large proportion must undoubtedly exercise considerable influence on the therapeutic action of the water ; and therefore it is highly desirable that the determination of its amount should be repeated from time to time.

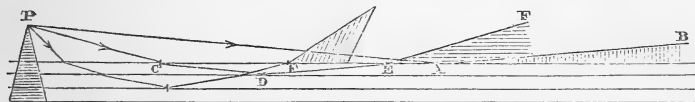
VII. *Remarkable Atmospheric Phenomena in Ceylon.* By  
the Rev. R. ABBAY, M.A.\*

ONE of the most startling and extraordinary of all atmospheric phenomena may be seen during fine weather from the summit of Adam's Peak in Ceylon. This mountain rises extremely abruptly from the low country, and by its elevation of 7200 feet above the sea commands a most extensive and map-like view of all the low lands to the south-west and north-west for a distance of 50 miles or more up to the sea coast. The upper part of the mountain is an acute cone of solid rock, some 2000 feet or more in height, that rises in perfect isolation above the range of which it forms a part, and also above all the neighbouring mountains to the east and north. The phenomenon, which is described by all who have witnessed it as of the most striking character, is seen at sunrise, and consists *apparently* of an enormous elongated shadow of the mountain projected to the westward, not only over the land but over the sea, to a distance of 70 or 80 miles. As the sun rises higher it rapidly approaches the mountain, and appears at the same time to rise before the spectator in the form of a gigantic pyramid of shadow. Distant objects, a hill or a river (or even Colombo itself, at a distance of 45 miles), may be distinctly seen through it, lighted up by the sunlight, diffused most probably by the surrounding illuminated atmosphere ; so that the shadow is not really a shadow on the

\* Communicated by the Physical Society (read May 27).

land, but a veil of darkness suspended between the observer and the low country. All this time it is rapidly rising and approaching, and each instant becoming more distinct, until suddenly it seems to fall back on the spectator, like a ladder that has been reared beyond the vertical; and the next instant it is gone. Of the accuracy of the above facts I have no doubt whatever. A great number of trustworthy witnesses have described it to me; and but for a sudden attack of fever at the foot of the peak, which prevented an ascent being made in the evening, I should have been able to describe it from personal observation. As it was I ascended next day and was able to form a very good idea of the conditions under which the phenomenon takes place; but I was, of course, unable to obtain accurate data as to the duration of the veil, the height to which its apex rises above the horizontal, the elevation of the sun, &c. If I am right in supposing that no explanation has ever been offered of this remarkable appearance, the following remarks, which appear to be fairly satisfactory, may perhaps not be uninteresting. The average temperature at night in the low country, during the dry season when Europeans ascend the mountain, is between  $70^{\circ}$  and  $80^{\circ}$  F., whilst that on the summit of the peak is from  $30^{\circ}$  to  $40^{\circ}$  F. Consequently the lower strata of air are much less dense than the upper; and an almost horizontal ray of light passing over the summit, must of necessity be refracted upwards and suffer total internal reflection as in the case of an ordinary mirage. This may be readily seen by a reference to fig. 1, where a

Fig. 1.

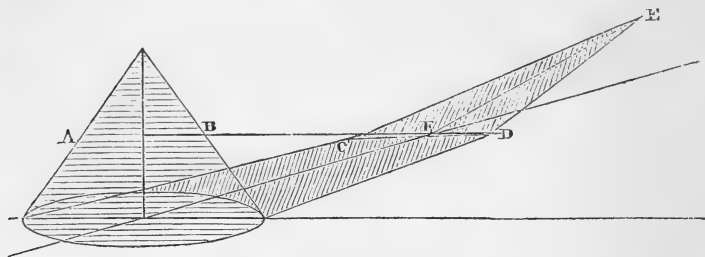


nearly horizontal ray passing over the summit of the peak P suffers total internal reflection at A, and is deviated upwards in the direction A B; or when a less horizontal ray is refracted at C, reflected at D, and refracted again at E, it finally issues along the line E F. It will be evident from fig. 1 that the shaded parts represent the veil of darkness at three different moments of time, and also that the veil appears to rise as its base approaches the mountain, *i. e.* as the sun rises and the rays fall less horizontally.

By referring to fig. 2 it will be seen how the aerial shadow of the upper part of the peak, *i. e.* the part above A B, suffers total internal reflection, and is thrown upwards into the air as C E D. It will also be evident, by comparing the two figures,

that as the veil of darkness approaches it must tend to assume a more vertical position until it reaches the critical angle

Fig. 2.



when total internal reflection ceases and the veil suddenly disappears. Its apparent tilting over on the spectator I imagine to be merely an illusion produced by the rapid approach and rising of the dark veil without any gradual disappearance which can be watched and estimated. It will be evident that it is the illumination of the innumerable particles floating in the atmosphere, and capable of reflecting light, that causes this aerial shadow to be visible by contrast.

Another atmospheric phenomenon visible in Ceylon, of great interest but not so striking in character as the above, admits of an equally simple explanation. Not unfrequently in the mountain districts broad beams apparently of bluish light may be seen extending from the zenith downwards, converging and narrowing as they approach the horizon. This ray-like appearance is very similar to that seen before sunrise; only the point from which the rays proceed is *opposite* the sun: the rays themselves are very broad and blue in colour; and the spaces between them have the ordinary illumination of the rest of the sky. If we suppose in this instance that the lower strata of air are colder than the upper (a condition of the atmosphere which not unfrequently occurs in a tropical mountain district like that of Ceylon, where large currents of heated air sweeping up a valley cross another valley nearly at right angles and at a considerable elevation above it), the refraction spoken of in the case of Adam's Peak will be downwards instead of upwards. If, too, the observer be *below*, the veil of darkness will appear to him like a very elongated triangle apex downwards, or broad ray, through which the blue sky beyond may be seen free from the palish illumination of the atmosphere, whilst on either side the ordinary illuminated sky will be seen. If now we suppose several isolated masses of cloud to partially obscure the sun, as was the case when I witnessed the phenomenon,



we may have several corresponding inverted veils of darkness, like blue rays in the sky, all apparently converging towards the same point below the horizon. This apparent convergence of the beams is merely an effect of distance, as in the case of parallel rays of light from the rising or setting sun, the blue rays being practically parallel bands in the atmosphere devoid of illumination. It will be evident that conical-shaped clouds are not necessary to produce this effect. Isolated clouds of any massive form would be sufficient to throw the bands of shadow through the illuminated atmosphere, and refraction and perspective would do the rest. The above phenomenon is called by the Singhalese "Buddha's rays;" and though according to Sir Emerson Tennent it is very varied in character and appears in different parts of the sky, yet I have only seen it when the sun was low at evening and when the rays converged to a point, apparently directly opposite the sun; and I do not think it possible for the phenomenon to be seen in any other position.

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VIII. *Formation of the Ocean-beds by Deformation of the Spheroid.* By ROBERT MALLET, F.R.S.\*

IN a paper read before the Cambridge Philosophical Society, February 22, 1875, by the Rev. O. Fisher, entitled "On the Inequalities of the Earth's Surface as produced by Lateral Pressure, upon the hypothesis of a liquid substratum" (Camb. Phil. Trans. vol. xii. part 2), for a copy of which I have been quite recently indebted to the politeness of the author, I find the following:—"If solidification of our globe from a fluid state commenced at the surface, the amount of radial contraction in the solid parts beneath the surface of the mountain-region has been less than in the parts beneath the sea-bed. In fact it is this unequal contraction which appears to have caused the hollows in the external surface, which have become the basins into which the waters have flowed to form the ocean." These views of Archdeacon Pratt's (*Figure of the Earth*, 1871) appear to be adopted by the author, who proceeds:—"Mr. Mallet, in his paper on Volcanic Energy (see Phil. Trans. Roy. Soc. 1873, part 1, paragraphs 52 to 56), takes a similar view. He thinks that the land and sea boundaries were shaped out by radial contraction during the first great stage of the operation of refrigeration, while the crust was thin and flexible, owing to the rapid contraction of its viscous portion which must then have been much thicker than the solid sheet above it." It seems to me that I am thus, when taken in connexion with the rest of

\* Communicated by the Author.

the Rev. O. Fisher's paper, made responsible for what I have never expressed. I have indeed stated that at an early stage in the cooling of our planet, contraction was met mainly by that deformation of the spheroid which gave rise to the continents and ocean-beds; but I have abstained from assigning any mechanism for the immediate production of that deformation, which I still believe the state of our knowledge does not enable us fully to explain. Difference of coefficient of contraction beneath land and ocean is not any necessary condition to the deformation of the spheroid; and had I ventured to speculate upon the mechanism of such deformation, I certainly should not have rested it upon so extravagant and unsustainable an hypothesis as that all parts of our globe beneath the ocean had a larger coefficient of contraction than those beneath the land—one the deductions from which would be quite irreconcilable with even what we already know as to the forms of both continents and ocean-beds.

The actual mechanism of deformation here referred to is far too complex to be attributable to a single condition such as that of difference of contractility beneath the sea and land; and whenever its solution shall be arrived at, it will be found to have involved considerations of the most diverse order, some of which I have pointed out in paragraphs 55 and 56 and elsewhere in my paper above referred to.

June 19, 1876.

### IX. Notices respecting New Books.

*Algebra for Beginners.* By JAMES LOUDON, M.A., Professor of Mathematics and Natural Philosophy, University College, Toronto. Toronto: Copp, Clark & Co., Front Street, 1876. (12mo, pp. 158.)

*The Elements of Algebra, originally designed for the use of Students in the Universities.* By JAMES WOOD, D.D., formerly Dean of Ely, and Master of St. John's College, Cambridge. New Edition (the Seventeenth). Remodelled, Simplified, and adapted for general use in Schools—with numerous Easy Exercises, Examples, and University Examination Papers. By THOMAS LUND, B.D., Prebendary of Lichfield, Rector of Brindle, Lancashire, and formerly Fellow and Lecturer of St. John's College, Cambridge. London: Longman, Green & Co., 1876. (Small 8vo, pp. 604.)

THESE works are almost sufficiently described by their title-pages. The former is a very elementary treatise, and will take the beginner to quadratic equations inclusive. The last chapter is devoted to an explanation of Exponential Notation. It seems to have been carefully drawn up, and is illustrated by many easy examples. It will doubtless prove useful to the beginners whom, we presume, the author has to teach.

The second work is one of more pretensions. The original algebra of Dr. Wood was published at the end of last century, and was characterized by a most competent judge (the late Dr. Peacock) as an admirable specimen of simple exposition, combined with adequate but not excessive illustration. As time went on, however, parts of the work needed expansion; and (we believe about 1840) Mr. Lund published an appendix to it, consisting of additional propositions and examples. In 1845 he incorporated this appendix with the original work, some parts of which he omitted, *e. g.* that on the theory of Equations. The present edition differs from that of 1845 in several minor respects, but embraces nearly the same topics. Perhaps the chief difference between the editions is that the present contains a much larger number of examples than the former. The addition is made in three ways, *viz.* by appending to each section of the work numerous easy examples, by increasing the number of harder examples at the end of the book, and by adding three series of Miscellaneous Examples, and twenty-four papers of Questions in Algebra which have been recently set in University Examinations at Cambridge.

It marks a real advance in Education that the present work, originally designed for Students in the University, should have to be adapted, and on the whole with large additions, for use in Schools, because the subject is now almost universally taught in them. At the same time we cannot help feeling a doubt whether it is not a mistake to insert in elementary mathematical books the enormous number of examples which they now ordinarily contain. Their insertion is, of course, a consequence of the effort which teachers have to make in order to qualify their pupils for competing with the ingenuity of examiners. Surely, however, if the question were merely one of education, a lad of seventeen or eighteen might with advantage have his attention directed to other subjects long before practice had made him pretty certain of solving such an equation as

$$x^3 - \sqrt[3]{6} x = 1$$

by his own lights and without a knowledge of Cardan's Rule.

## X. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from vol. i. p. 326.]

Jan. 13, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

**T**HE following paper was read:—

“On the Optical Department of the Atmosphere in reference to the Phenomena of Putrefaction and Infection.” By John Tyndall, F.R.S.

The author refers in an Introduction to an inquiry on the decomposition of vapours and the formation of active clouds by light,

whereby he was led to experiment on the floating matter of the air. He refers to the experiments of Schwann, Schröder, and Dusch, Schröder himself, to those of the illustrious French chemist Pasteur, to the reasoning of Lister and its experimental verification regarding the filtering-power of the lungs; from all of which he concluded, six years ago, that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand. He thought the simple expedient of examining by means of a beam of light, while the eye was kept sensitive by darkness, the character of the medium in which their experiments were conducted could not fail to be useful to workers in this field. But the method has not been much turned to account; and this year he thought it worth while to devote some time to the more complete demonstration of its utility.

He also wished to free his mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation." Pasteur has pronounced it "a chimera," and expressed the undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question is one of the last importance. But the state of medical opinion regarding it is not satisfactory. In a recent Number of the 'British Medical Journal,' and in answer to the question, "In what way is contagium generated and communicated?" Messrs. Braidwood and Vacher reply that, notwithstanding "an almost incalculable amount of patient labour, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease-germs, are always produced from like bodies previously existing, or whether they do not, under certain favourable conditions, spring into existence *de novo*."

With a view to the possible diminution of the uncertainty thus described, the author submits without further preface to the Royal Society, and especially to those who study the ætiology of disease, a description of the mode of procedure followed in this inquiry and the results to which it has led.

A number of chambers, or cases, were constructed, each with a glass front, its top, bottom, back, and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole 2 inches in diameter, closed air-tight by a sheet of india-rubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar, 2 inches in diameter and  $1\frac{1}{2}$  inch high, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the india-rubber, but it also passes through a stuffing-box of sticky cotton-wool. The width

of the aperture closed by the india-rubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the cupboard are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are bent several times up and down, so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air.

The bottom of the box is pierced, sometimes with two rows, sometimes with a single row of apertures, in which are fixed, air-tight, large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

On the 10th of September the first case of this kind was closed. The passage of a concentrated beam across it through its two side windows then showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered and after it quitted the case its track was vivid in the air, but within the case it vanished. Three days of quiet sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air. During ebullition aqueous vapour rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature, through the bent tubes at the top. Before the brine was removed little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling chamber should at first be forcible enough to carry motes along with it. As soon, however, as the ambient temperature was assumed by the air within the case the cotton-wool stoppers were removed.

We have here the oxygen, nitrogen, carbonic acid, ammonia, aqueous vapour, and all the other gaseous matters which mingle more or less with the air of a great city. We have them, moreover, "untortured" by calcination and unchanged even by filtration or manipulation of any kind. The question now before us is, can air thus retaining all its gaseous mixtures, but self-cleansed from mechanically suspended matter, produce putrefaction? To this question both the animal and vegetable worlds return a decided negative.

Among vegetables experiments have been made with hay, turnips, tea, coffee, hops, repeated in various ways with both acid and alkaline infusions. Among animal substances are to be mentioned many experiments with urine; while beef, mutton, hare, rabbit, kidney, liver, fowl, pheasant, grouse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, oyster have been all subjected to experiment.

The result is that infusions of these substances exposed to the common air of the Royal Institution laboratory, maintained at a

temperature of from  $60^{\circ}$  to  $70^{\circ}$  Fahr., all fell into putrefaction in the course of from two to four days. No matter where the infusions were placed, they were infallibly smitten in the end. The number of the tubes containing infusions was multiplied till it reached six hundred, but not one of them escaped infection.

In no single instance, on the other hand, did the air which had been proved moteless by the searching beam prove itself, even when raised to temperatures varying from  $80^{\circ}$  to  $90^{\circ}$ , to possess the least power of producing Bacterial life or the associated phenomena of putrefaction. The power of developing such life in atmospheric air, and the power of scattering of light, are thus proved to be indissolubly united.

The sole condition necessary to cause these long-dormant infusions to swarm with active life is the access of the floating matter of the air. After having remained for four months as pellucid as distilled water, the opening of the back door of the protecting case, and the consequent admission of the mote-laden air, sufficed in three days to render the infusions putrid and full of life.

That such life arises from mechanically suspended particles is thus reduced to ocular demonstration.

Let us inquire a little more closely into the character of the particles which produce the life. Pour eau de Cologne into water, a white precipitate renders the liquid milky. Or, imitating Brücke, dissolve clean gum mastic in alcohol, and drop it into water, the mastic is precipitated, and milkiness produced. If the solution be very strong the mastic separates in curds; but by gradually diluting the alcoholic solution we finally reach a point where the milkiness disappears, the liquid assuming, by reflected light, a bright cerulean hue. It is, in point of fact, the colour of the sky, and is due to a similar cause, namely, the scattering of light by particles, small in comparison to the size of the waves of light.

When this liquid is examined by the highest microscopic power it seems as uniform as distilled water. The mastic particles, though innumerable, entirely elude the microscope. At right angles to a luminous beam passing among the particles they discharge perfectly polarized light. The optical deportment of the floating matter of the air proves it to be composed in part of particles of this excessively minute character. When the track of a parallel beam in dusty air is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finer matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that the author shows that the matter potential as regards the development of Bacterial life is to be sought.

But though they are beyond the reach of the microscope, the existence of these particles, foreign to the atmosphere but floating in it, is as certain as if they could be felt between the fingers or seen by the naked eye. Supposing them to augment in magnitude

until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles, and that in forty-eight hours subsequently buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment when repeated over and over again to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust or mineral particles, or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment with the flower-pot as clearing up our preexisting ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been “spontaneously generated.”

This reasoning applies word for word to the development of *Bacteria* from that floating matter which the electric beam reveals in the air, and in the absence of which no Bacterial life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of Bacterial life developed from dead dust can ever gain currency among the members of a great scientific profession.

A novel mode of experiment has been here pursued, and it may be urged that the conditions laid down by other investigators in this field, which have led to different results, have not been strictly adhered to. To secure accuracy in relation to these differences, the latest words of a writer on this question, who has materially influenced medical thought both in this country and in America, are quoted. “We know,” he says, “that boiled turnip- or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days.” Who the “we” are who possess this knowledge is not stated. The author is certainly not among the number, though he has sought anxiously for knowledge of the kind. He thus tests the statements in succession.

And first, with regard to filtered air. A group of twelve large test-tubes was caused to pass air-tight through a slab of wood. The wood was coated with cement, in which, while hot, a heated “propagating-glass” resembling a large bell-jar was imbedded. The air within the jar was pumped out several times, air filtered through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton—three of each, twelve in all. They are as clear and cloudless at the present moment as they were upon the day of their introduc-

tion: while twelve similar tubes, prepared at the same time, in precisely the same way, and exposed to the ordinary air, are clogged with mycelium, mould, and *Bacteria*.<sup>3</sup>

With regard to the calcined air, a similar propagating-glass was caused to cover twelve other tubes filled with the same infusions. The "glass" was exhausted and carefully filled with air which had passed through a red-hot platinum tube containing a roll of red-hot platinum gauze. Tested by the searching beam, the calcined air was found quite free from floating matter. Not a speck has invaded the limpidity of the infusions exposed to it, while twelve similar tubes placed outside have fallen into rottenness.

The experiments with calcined air took another form. Six years ago it was found that, to render the laboratory air free from floating matter, it was only necessary to permit a platinum wire heated to whiteness to act upon it for a sufficient time. Shades containing pear-juice, damson-juice, infusions of hay and turnip, and water of yeast were freed from their floating matter in this way. The infusions were subsequently boiled and permitted to remain in contact with the calcined air. They are quite clear to the present hour, while the same infusions exposed to common air became mouldy and rotten long ago.

It has been affirmed by other writers on this question that turnip- and hay-infusions rendered slightly alkaline are particularly prone to exhibit the phenomena of spontaneous generation. This was not found to be the case in the present investigation. Many such infusions have been prepared, and they have continued for months without sensible alteration.

Finally, with regard to infusions wholly withdrawn from air, a group of test-tubes containing different infusions was boiled under a bell-jar first filled with filtered air, and from which the air was subsequently removed as far as possible by a good air-pump. They are now as pellucid as they were at the time of their preparation more than two months ago, while a group of corresponding tubes exposed to the laboratory air have all fallen into rottenness.

There is still another form of experiment on which great weight has been laid—that of hermetically sealed tubes. On the 6th of last April a discussion on the "Germ-Theory of Disease" was opened before the Pathological Society of London. The meeting was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The following important summary of these was then given by Dr. Bastian:—"With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blowpipe-flame, we may boil the fluid, seal the vessel during ebullition, and, keeping it in a warm place, may await the result, as I have often done. After a variable time, the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with *Bacteria* and allied organisms."



Previous to reading this statement the author had operated upon 16 tubes of hay- and turnip-infusions, and upon 21 tubes of beef, mackerel, eel, oyster, oatmeal, malt, and potato, hermetically sealed while boiling, not by the blowpipe, but by the far more handy spirit-lamp flame. In no case was any appearance whatever of *Bacteria* or allied organisms observed. The perusal of the discussion just referred to caused the author to turn again to muscle, liver, and kidney, with a view of varying and multiplying the evidence. Fowl, pheasant, snipe, partridge, plover, wild duck, beef, mutton, heart, tongue, lungs, brains, sweetbread, tripe, the crystalline lens, vitreous humour, herring, haddock, mullet, codfish, sole, were all embraced in the experiments. There was neither mistake nor ambiguity about the result. One hundred and thirty-nine of the flasks operated on were submitted to the Fellows; and not one of this cloud of witnesses offered the least countenance to the assertion that liquids within flasks, boiled and hermetically sealed, swarm, subsequently, more or less plentifully with *Bacteria* and allied organisms.

The evidence furnished by this mass of experiments, that Dr. Bastian must have permitted errors either of preparation or observation to invade his work, is, it is submitted, very strong. But to err is human; and in an inquiry so difficult and fraught with such momentous issues, it is not error, but the persistence in error by any of us for dialectic ends that is to be deprecated. The author shows by illustrations the risks of error run by himself. On the 21st of October he opened the back door of a case containing six test-tubes filled with an infusion of turnip which had remained perfectly clear for three weeks, while three days sufficed to crowd six similar tubes exposed to mote-laden air with *Bacteria*. With a small pipette he took specimens from the pellucid tubes, and placed them under the microscope. One of them yielded a field of Bacterial life, monstrous in its copiousness. For a long time he tried vainly to detect any source of error, and was perfectly prepared to abandon the unvarying inference from all the other experiments, and to accept the result as a clear exception to what had previously appeared to be a general law. The cause of his perplexity was finally traced to the tiniest speck of an infusion containing *Bacteria* which had clung by capillary attraction to the point of one of his pipettes.

Again, three tubes containing infusions of turnip, hay, and mutton were boiled, on the 2nd of November, under a bell-jar containing air so carefully filtered that the most searching examination by a concentrated beam failed to reveal a particle of floating matter. At the present time every one of the tubes is thick with mycelium and covered with mould. Here, surely, we have a case of spontaneous generation. Let us look to its history.

After the air has been expelled from a boiling liquid it is difficult to continue the ebullition without "bumping." The liquid remains still for intervals and then rises with sudden energy. It did so in the case now under consideration; and one of the tubes boiled over, the liquid over spreading the resinous surface in which

the bell-jar was imbedded. For three weeks the infusions had remained perfectly clear. At the end of this time, with a view of renewing the air of the jar, it was exhausted, and refilled by fresh air which had passed through a plug of cotton-wool. As the air entered, two small spots of *Penicillium*, resting on the liquid which had boiled over, attracted attention. It was at once remarked that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the *Penicillium* and diffuse them in the bell-jar. This was therefore filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes, namely that containing the hay-infusion. It has by this time grown so as to fill a large portion of the tube. For nearly a month longer the two tubes containing the turnip- and mutton-infusions maintained their transparency unimpaired. Late in December the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft upon its surface. The beef-infusion continued bright and clear for nearly a fortnight longer. The recent cold weather caused the author to add a third gas-stove to the two which had previously warmed the room in which the experiments are conducted. The warmth played upon one side of the bell-jar, causing currents within it; and the day after the lighting of the stove, the beef-infusion gave birth to a tuft of mycelium. In this case the small spots of *Penicillium* might have readily escaped attention; and had they done so we should have had three cases of "spontaneous generation" far more striking than many that have been adduced.

In further illustration of the danger incurred in this field of inquiry, the author refers to the excellent paper of Dr. Roberts on Biogenesis, in the Philosophical Transactions for 1874. Dr. Roberts fills the bulb of an ordinary pipette up to about two thirds of its capacity with the infusion to be examined. In the neck of the pipette he places a plug of dry cotton-wool. He then hermetically seals the neck, and dips the bulb into boiling water or hot oil, where he permits it to remain for the requisite time. Here we have no disturbance from ebullition and no loss by evaporation. The bulb is removed from the hot water and permitted to cool. The sealed end of the neck is then filed off, the cotton-wool alone interposing between the infusion and the atmosphere.

The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro, as the temperature changed, or by any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool which, falling into the infusion, would produce its effect. Probably also condensation occurred at times in the neck of the pipette, the water of condensation carrying back from the cotton-wool the seeds of life. The fact of fertilization being so rare as Dr. Roberts found it to be, is a proof of the care with which his

experiments were conducted. But he did find cases of fertilization after prolonged exposure to the boiling temperature; and this caused him to come to the conclusion that under certain rare conditions spontaneous generation may occur. He also found that an alkalized hay-infusion was so difficult to sterilize that it was capable of withstanding the boiling temperature for hours without losing its power of generating life. The most careful experiments have been made with this infusion. Dr. Roberts is certainly correct in assigning to it superior nutritive power. But in the present inquiry five minutes' boiling sufficed to completely sterilize the liquid.

Summing up this portion of his inquiry, the author remarks that he will hardly be charged with any desire to limit the power and potency of matter in regard to life. But holding the notions he does upon this point, it is all the more incumbent on him to affirm that, as far as inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life.

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GEOLOGICAL SOCIETY.

[Continued from p. 563.]

May 10, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On some Fossil Reef-building Corals from the Tertiary deposits of Tasmania." By Prof. P. Martin Duncan, M.B., F.R.S., President.

2. "On the Echinodermata of the Australian Cainozoic (Tertiary) Deposits." By Prof. P. Martin Duncan, M.B., F.R.S., President.

3. "On the Miocene Fossils of Haiti." By R. J. Lechmere Guppy, Esq., F.L.S., F.G.S.

May 24, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the old glaciers of the northern slope of the Swiss Alps." By Prof. Alphonse Favre, F.M.G.S.

The author illustrated his remarks by a map on a scale of  $\frac{1}{250000}$ , showing the space occupied by the old Swiss glaciers at the time of their greatest extension, and founded in part upon evidence obtained since 1867, when he, in conjunction with Prof. Studer and M. L. Soret, issued an "Appel aux Suisses" for the preservation of erratic blocks. He said that in existing glaciers two parts may be recognized—an upper one, the reservoir or feeding glacier, and a lower one, the flowing glacier. Applying this division to the old glaciers, it appears that in the glaciers of the Rhone and Rhine the

flowing glacier (which occupied the plain) had a surface nearly equal to that of the feeding glacier (which was situated in the mountains). By means of several tables M. Favre showed the height attained by these glaciers, their thickness, the slope of their upper surface &c. at various points in the Alps, the Jura, and Swabia, and deduced as the result of the comparison of these numbers:—1. that the Rhone glacier passed over several of the chains of the Jura, and that the ice covering these, far from being an obstacle to the extension of the glaciers of the Alps, actually reinforced them, and served them as *relays*, the glaciers of the Jura having carried far on the Alpine erratic blocks; 2. that the slopes of the upper surface were variable, and were null, or nearly so, over considerable spaces.

At the Calanda, near Coire, there are erratics which seem to be at a higher level than that attained by the glacier. This may be explained by the formation in the glacier of a sort of *eddy*, which would elevate the ice to a certain amount over a limited space.

During their greatest extension the Swiss glaciers came into contact with those of central France near Lyons; they united with those of the Jura, the Black Forest, and the Austrian and Italian Alps; they stretched from the plain of the Po to that of the Danube; and, further, for distances of 50 or 100 kilometres they nearly approached horizontality. Hence they resembled the glaciers of the interior of Greenland and Spitzbergen, so far as can be judged from the descriptions.

2. "Evidences of Theriodonts in Permian deposits elsewhere than in South Africa." By Prof. R. Owen, C.B., F.R.S., F.G.S.

## XI. *Intelligence and Miscellaneous Articles.*

LETTER IN REPLY TO FATHER SECCHI'S LETTER ON THE DISPLACEMENT OF THE STELLAR LINES. BY WILLIAM HUGGINS, D.C.L., LL.D., F.R.S.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

**M**AY I ask you to insert in the next Number of the Philosophical Magazine the following translation of a letter which I have addressed to MM. les Secrétaires Perpétuels of the Académie des Sciences in reply to the letter from Father Secchi, of which a translation appeared in the Supplementary Number of your Magazine for June?

Yours &c.,

June 7, 1876.

WILLIAM HUGGINS.

I hesitate to occupy the time of the Academy with a few words in reply to a letter from Father Secchi, read on April 3, especially because Mr. Christic, of Greenwich, has communicated to the Royal Astronomical Society a summary of the results recently observed at

Greenwich, which agree in a remarkable way with my observations on the same stars. It is true that there were discordances among the early Greenwich observations; but these were due for the most part to the apparatus, which was not in all respects sufficiently trustworthy. Mr. Christie compares his results with mine in the following Table. The figures indicate the velocity of approach or of recession in English miles.

A. Comparison of Star Motions in line of Sight.

+ = recession. - = approach.

Star.	Huggins..	Greenwich.
$\alpha$ Andromedæ...	—	—35
Aldebaran .....	+ ?	+
Capella .....	+	+20
Rigel .....	+	+
Betelgeuse .....	+22	+75
Sirius .....	+18-29	+25
Castor .....	+23-28	+25
Procyon .....	+	+40
Pollux .....	-49	—
Regulus .....	+12-17	+30
$\beta$ Ursæ Majoris	+17-21	+
$\alpha$ Ursæ Majoris	-46-60	—
$\beta$ Leonis .....	+ ?	—
Spica .....	+	+
$\eta$ Ursæ Majoris	+ ?	—
Arcturus .....	-55	-35
Boötes .....	- ?	—
$\alpha$ Coronæ.....	+	+
Vega .....	-44-54	-37
$\alpha$ Cygni .....	-39	-50
$\alpha$ Pegasi .....	—	-27

Mr. Christie remarks :—"Notwithstanding these difficulties, it is gratifying to find that out of the list of twenty-one stars which have been observed both by Dr. Huggins and Mr. Maunder there are only two cases of discordance, and for both these stars Dr. Huggins has expressed himself as dissatisfied with his observations; whilst the Greenwich results for these stars rest on too few observations at present."

Since this paper was presented to the Royal Astronomical Society, the displacement of the Fraunhofer lines in the spectrum of Venus has been observed at Greenwich; and it agrees in direction with the known movement of that planet.

It is not necessary for me to investigate the causes of Father Secchi's want of success. It is sufficient to say that from the first I considered the obtaining of the assurance that the comparison of the star-spectrum with that of a terrestrial substance was not in the very least affected by the motions of the telescope to be obviously one of the very first and most necessary precautions to take.

Father Secchi writes :—" Nous acquimes la conviction que la raie pouvait paraître constamment d'un côté ou de l'autre, selon la disposition de l'instrument sans que l'observateur eût un indice assez sûr pour reconnaître l'illusion dont il était victime." Such an illusion was scarcely possible in the method which I employed, because from the first I was careful to add to other necessary precautions, that of directing frequently the telescope with the spectroscope attached, to the moon, when the triple line *b* always coincided perfectly with the three bright lines of magnesium. When the instrument, without any possible derangement, was again directed to the star, the same displacement of the line was seen which had been observed previously.

It is also not necessary for me to state the special precautions which have to be taken when a stellar line "très-large et estampée à son bord" has to be compared with the bright line  $\beta$  of hydrogen, because many of the comparisons were made with narrow and defined stellar lines with the lines of magnesium and of sodium. The displacement due to the approach of Arcturus was observed not only in the case of the line F with hydrogen, but also, in the case of the triple line *b*, with the three lines of magnesium, and the double line D with the two lines of sodium.

The observations of the displacements of lines in the spectra of stars are necessarily, from their nature, of extreme delicacy; and success can be expected only when suitable care and precautions are taken.

Father Secchi says, p. 762, "M. Huggins reprit peu après la question." The application of the spectroscope to the movements of the heavenly bodies was entirely original on my part. It is true that Father Secchi's letter describing his negative results appeared in the *Comptes Rendus* for March 2, 1868, while my paper was presented to the Royal Society the following month, April 23, 1868; but my observations for the most part, as is stated in my paper, were made during the preceding year. The idea of the method was familiar to Dr. Miller and myself at the time of our comparisons of stellar with terrestrial spectra in 1862-63: see Phil. Trans. 1868, p. 529.

#### ON THE INTERNAL CONSTITUTION OF MAGNETS. BY J. JAMIN.

In a saturated magnet of infinite length, the magnetic intensities  $y$  are expressed by the formula of  $y = Ak^{-x}$ , which represents a curve the more elongated as  $k$  is smaller:  $\frac{1}{k}$  is what I have named the *coefficient of conductivity*. For one and the same quality of steel in its different states  $A$  is invariable; but the conductivity increases with the annealing, and takes its least value when the steel has been dipped at a white heat in cold water. If now different kinds of tempered steel are compared, we find that, the higher the degree of acieration, the less is this minimum of conductivity. But as the

commercial kinds do not contain more than 1 per cent. of carbon, I had prepared by M. Dalifol a series of steels which have been submitted to repeated cementations, then cast, in which the proportion of carbon progressively increases, and the last members of the series will not bear the forge. Chilled at a white heat they are more and more hard, not attacked by acids except aqua regia, less and less conductors of magnetism, and, what is more, less and less magnetic. The last members are scarcely attracted by an electromagnet, scarcely magnetized by a powerful coil either during or after the passage of the current (*Comptes Rendus*, vol. lxxvii. p. 91).

I have explained these facts by assuming that those steels which are bad conductors of magnetism in the direction of their length are the same also in the direction of their depth—that their magnetization, considerable at the surface, decreases thence to the centre, and that for the most cemented the magnetized layer does not exceed the thickness of a varnish. I have, besides, established that the law of the decrease of magnetism is given by the preceding formula. These ideas having been recently contested on the ground of some experiments neither very precise nor correctly interpreted, I proceed to defend them and give them precision by more thorough investigation.

I take as an example a bar of steel prepared by M. Dalifol, very homogeneous, well tempered, very hard, and having very little conductivity. Its dimensions were—length 280 millims., breadth 50 millims., and thickness 10·6 millims. After magnetizing it to saturation, and keeping it at rest for some days to bring it to its definitive equilibrium, I dissolved it gradually in cold aqua regia, taking it out from time to time to measure its thickness  $e$  and its total magnetism  $M$ , continuing the operation without interruption, as rapidly as possible, during one whole day. Here are the results:—

$e$ .	$M$ .	Difference for 0·1 millim.	Fraction removed.	
			Thickness.	Magnetism.
millim.	millim.			
10·6	25·00			
10·5	22·90	2·10	0·009	0·084
10·4	21·26	1·64	0·018	0·142
10·3	20·05	1·21	0·028	0·198
10·2	19·00	1·05	0·037	0·240
10·0	17·25	0·85	0·056	0·310
9·9	16·62	0·63	0·066	0·335
9·4	14·08	0·43	0·113	0·436
8·9	12·20	0·35	0·160	0·512
8·6	11·45	0·25	0·188	0·550
8·4	11·00	0·20	0·207	0·564

The values of  $M$  sensibly satisfy the equation  $M = Ae + Be^3$ . The third column contains the losses of magnetism for successive diminutions of thickness equal to 0·1 millim. These losses, at first equal

to 2.1, diminish regularly down to 0.2; therefore the density of the magnetic layer diminishes from the surface to the centre.

The fourth column contains the fractions of thickness removed; the fifth, the fractions of magnetism vanished. These latter are always greater than the former. When the thickness is diminished  $\frac{1}{15}$ , the magnetism becomes  $\frac{5}{15}$  less; if we take away  $\frac{1}{6}$  of the thickness, we remove half of the magnetism. In other terms, in consuming on each side  $\frac{2}{10}$  of a millim. we take one fourth of the total magnetism, and for 1.1 millim. nearly two thirds. Therefore the two thirds of the magnetism were confined within a layer of 1.1 millim. thickness, enveloping a nucleus of 8.4 millims. which preserves about one third only of the total magnetism.

If we remagnetize this nucleus of 8.4 millims., we give it a total magnetism equal to 23.5, nearly as much as the original bar possessed before its wasting. We see that by this remagnetization a new magnetic layer is produced at the surface to replace that which was taken away, and is nearly equal to it, can be itself taken away like the first and be replaced like it in the subjacent thicknesses.

In steel still more cemented the magnetism intrenches itself within a thickness still less; but in the commercial kinds of steel, which are much more conductive, the magnetism penetrates almost uniformly the entire mass, as might be easily foreseen\*.

These results are in harmony with the theory. If it be admitted that, in a bar of thickness  $2E$ , starting from the two faces the magnetism diminishes according to the same law as in the direction of the length, we find that it is expressed by the formula

$$y = A(k^{-(E-x)} + k^{-(E+x)});$$

$x$  is counted from the middle of the bar. To get the total quantity  $M$  of magnetism comprised within this bar from  $-e$  to  $+e$  (that is to say, reduced to the thickness  $2e$ ),  $ydx$  must be integrated from  $+e$  to  $-e$ , which gives

$$M = \frac{2A}{l.k} k^{-E} k^e - k^{-e}; \dots\dots\dots (1)$$

\* This is the case of MM. Trève and Durassier. They measure the quantity of magnetism by the sine of the deviation given by the magnet to a compass near it. All physicists know that this method measures nothing when the compass is close and the deviation great. But if we admit it to be a good method, it will at least require accurate calculation. The quantity of magnetism would be expressed not by the sine, but by the tangent of the deviation, as in the *tangent*-compass. On correcting this error of calculation in the experiments of MM. Trève and Durassier, it becomes evident that the magnetism is not proportional to the weight of the steel, that it is represented not by a right line, but by a curve, and that the magnetism is denser at the surface than at the centre. In brief, (1) the magnetism is not proportional to the weight of the steel; (2) the method employed was inadequate; (3) there is an error in the calculation of the experiments; (4) when it is corrected, results are found conformable to mine.



or, developing in a series,

$$M = 4Ak^{-E} \left( e + \frac{e^3}{1.2.3} l^3 k + \dots \right), \dots (2)$$

a formula in conformity with the preceding measurements.

Nevertheless the experiment of the dissolution of a magnet is far from simple. Numerous experiments prove that the residual magnetism of the nucleus is a function of the duration of the more or less rapid dissolution, of the length of the steel, and of a multitude of perturbing circumstances. If the foregoing results suffice to prove the concentration of magnetism at the surface, they cannot serve for finding the exact law of its internal distribution; we shall therefore pursue another course.

I magnetize the bar after applying to its extremities, upon the two faces, two long armatures of soft iron, tightened by pressure-screws. It is known that it then receives, as if it were infinite, the whole quantity of magnetism which its mean girth can contain. I measure this magnetism, and then, having reduced the bar in aqua regia, repolished its surface at the grindstone, and replaced the armatures, I resume the magnetization and its measurement. I determine, therefore, at each operation thus effected, the sum of magnetism  $M$  taken, at saturation, by one and the same steel of infinite length and diminishing thickness  $E$ .

On the other hand, this quantity of magnetism is expressed by the formula (1). Supposing  $e = E$ , we have

$$M = \frac{2A}{l.k} (1 - k^{-2E}); \quad \frac{M}{1 - k^{-2E}} = \frac{2A}{l.k} = \text{constant.} \dots (3)$$

For determining the constant  $k$  it is sufficient to take upon the curve two values  $M$  and  $M_1$  corresponding to two thicknesses  $2E$  and  $E$ :—

$$M = \frac{2A}{l.k} (1 - k^{-2E}); \quad M_1 = \frac{2A}{l.k} (1 - k^{-E}).$$

Put  $k^{-E} = z$ ,  $k^{-2E} = z^2$ , and, dividing,

$$\frac{M}{M_1} = \frac{1 - z^2}{1 - z},$$

an equation of the second degree, the roots of which are:—the one,  $z' = 1$ , which does not satisfy the question; the other,

$$z'' = k^{-E} = \frac{M}{M_1} - 1.$$

Taking the millimetre as unit of thickness, the mean value of  $k$  has been found equal to 1.0853. The following Table shows, in accordance with the formula, that the quotient of  $M$  by  $1 - k^{-2E}$  is a constant quantity. We shall, in consequence, admit the law of diminution of magnetism in the interior expressed by formula (3).

E.	M.	$\frac{M}{1-k^{-2E}}$	E.	M.	$\frac{M}{1-k^{-2E}}$
10.9	51.0	61.11	6.8	40.0	58.57
10.5	49.5	60.07	6.2	38.0	59.22
9.9	49.22	61.06	5.7	36.0	58.99
9.5	48.1	60.69	5.2	34.0	58.66
9.10	46.0	59.10	4.9	33.0	59.39
8.5	45.5	60.25	4.1	28.7	58.24
8.4	44.6	59.37	4.4	31.0	59.61
8.15	44.1	59.49	3.7	26.7	58.49
7.6	43.0	60.07	2.95	22.6	58.61
7.2	40.5	57.26	2.50	20.0	59.00

On discussing that formula we see that if  $E$  increases,  $k^{-2E}$  diminishes and  $M$  increases. Therefore the magnetism augments with the thickness of the bar. From a sufficient thickness (the less as  $k$  is greater) the magnetism no longer augments with  $E$ , but takes a limiting value

$$M_1 = \frac{2A}{l.k}.$$

This limiting value is the less the greater  $k$  is or the steel less conductive; whence it follows that steels which are much cemented and chilled will be magnetized very little, and equally under all thicknesses. Those singular kinds of steel which are no longer magnetic are thus accounted for.

Developing  $k^{-E}$ , we can write

$$M = 4AE = \left(1 - \frac{El.k}{1.2} + \dots\right).$$

Supposing that  $E$  increases from  $E$  to  $nE$ ,

$$M = 4AnE \left(1 - \frac{nEl.k}{1.2} + \dots\right);$$

but if we superposed  $n$  plates of thickness  $E$ , we should have

$$M_1 = 4AnE \left(1 - \frac{El.k}{1.2} + \dots\right).$$

The ratio

$$\frac{M_1}{M} = \frac{2 - El.k + \dots}{2 - nEl.k + \dots}$$

increases with  $n$ ; whence we see that a bundle of  $n$  plates will always be more powerful than a bar of the same steel having the same dimensions; and the ratio will increase indefinitely with the number of the plates. This justifies the employment of thin plates in the construction of magnets.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 19–24.

## ON THE DETERMINATION OF THE SOLIDIFYING-TEMPERATURE OF LIQUIDS, AND IN PARTICULAR OF SULPHUR. BY D. GERNEZ.

The determination of the temperature at which the passage of a body from the solid to the liquid state is effected, or inversely, presents, notwithstanding its apparent simplicity, uncertainties which have only been dissipated for a relatively restricted number of substances, even when the change of state takes place abruptly—that is to say, when the solid becomes suddenly liquid through an infinitesimal change of temperature. The slowness with which the fusion of a body is produced in a bath at a constant temperature but little above the fusing-point, and the imperfect conductivity of the substances (which permits certain parts of the liquid to attain temperatures above that of the part not melted), have led physicists to substitute for the determination of the fusing-point that, *supposed* identical, of the point of solidification: only it often happens in this case that the measurements are found to be falsified in consequence of the phenomena of surfusion. As I will show, these phenomena can be utilized to determine the solidifying-temperature of liquids with a precision only limited by the patience of the experimenter.

For this purpose, into a glass tube 3 centims. in diameter, closed at one end, such a quantity of the solid is put that in the liquid state it forms a column 5 or 6 centims. in height; along the axis of the tube a thermometer, held by a cork, is arranged, the bulb of which, sufficiently small to have only a negligible effect on the temperature of the surrounding liquid, descends to within a few millims. of the bottom, without touching the tube. The fusion of the body is then determined by putting it into a bath of water or paraffin at a temperature a few degrees higher than the presumed point of solidification. When the body is entirely melted, the tube is brought into a bath at a constant temperature lower than the temperature sought; and we wait until the indications of two thermometers (one internal, the other external) differ only very little. The result is accelerated by turning the tube on its axis—which does not provoke solidification, unless in the movement the thermometer rubs against the sides of the tube bathed by the liquid. The body being thus in the state of surfusion, a fine glass needle with its extremity covered with a small quantity of the material in the pulverulent state is introduced through a second aperture in the cork, and this extremity brought into the liquid, the solidification of which is immediately determined. To urge on the phenomenon, the cork is rotated about its axis, which moves the stem and carries the crystalline germs cylindrically round the thermometer, the indications of which are then followed, it soon attaining a maximum certainly not higher than the solidifying-point, but possibly lower. The experiment is then recommenced, the preceding maximum being chosen for the temperature of the surrounding bath; and by operating in the same manner the solidification of the liquid is determined: it is found that the thermometer rises to a maximum higher than the previous one. After two or three trials of this sort, temperatures are arrived at which differ from one another by only a

negligible fraction of a degree: the highest of them is taken for the solidification-temperature of the substance, applying to it the correction arising from the circumstance that the whole of the stem of the thermometer is not bathed by the liquid.

I have turned to account the precision which this process permits, especially to elucidate the various peculiarities presented by the change of state of sulphur; and the following are the chief results at which I have arrived:—

The most simple results is that which relates to sulphur insoluble in sulphide of carbon, obtained by exhaustion from flowers of sulphur. The solidification of this variety takes place at  $114^{\circ}3$  C., whatever may be the temperature at which it was melted: thus, in all the experiments, I have not found more than  $\frac{1}{10}$  of a degree difference between the temperature of solidification of the sulphur which had been brought to ebullition and that at which the same substance solidifies after being heated to  $170^{\circ}$  only, or even  $121^{\circ}$ .

This constancy of the solidifying-point is not found in the other varieties. For octahedral sulphur the temperature of solidification is higher when it has been fused at the lowest possible temperature—for instance, at  $121^{\circ}$ ; in this case it reached  $117^{\circ}4$ . If the liquid has been heated to  $144^{\circ}$ , it is only  $113^{\circ}4$ ; it descends to  $112^{\circ}2$  for the sulphur kept five minutes at  $170^{\circ}$  (which renders it very viscous, and, according to M. Berthelot's experiments, produces the maximum of insoluble sulphur). Starting from this value, the temperature of solidification rises rapidly to  $114^{\circ}4$ , the solidifying-point of the sulphur which has been heated to various temperatures between  $200^{\circ}$  and  $447^{\circ}$ . This last value is sensibly the same as that which corresponds to the solidification of the insoluble sulphur.

In regard to prismatic sulphur, its temperature of solidification depends on its previous state. If it comes from insoluble sulphur, it behaves like this; nevertheless, when it is submitted to several successive fusions and solidifications, not much exceeding the fusing-temperature, the solidifying-point may rise more than one degree. In like manner, if it comes from octahedral sulphur its solidifying-temperature depends on the temperature to which it has been brought. Thus, if it comes from sulphur heated to  $170^{\circ}$  (the solidifying-point of which is  $112^{\circ}2$ ), and has been liquefied at about  $120^{\circ}$  or  $123^{\circ}$ , the temperature of solidification rises gradually each time, and after a sufficient number of fusions and crystallizations it becomes equal to  $117^{\circ}4$ .

Soft sulphur, flowers of sulphur, and roll sulphur conduct, as might have been expected, to results intermediate to those which I have noted for insoluble and octahedral sulphur, both of which enter into their constitution.

These peculiarities account for the diversity of the numbers given for the temperature of change of state of sulphur by observers whose skill there is no reason for calling in question. They also show how tenacious are the modifications resulting from the tempering of sulphur, since, in order to cause their disappearance, a considerable number of successive fusions and crystallizations are necessary.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxxii. pp. 1151–1153.

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[FIFTH SERIES.]

AUGUST.

XII. *On the Specific Heats of Gases.*

By EILHARD WIEDEMANN\*.

[Plate I.]

SINCE the publication of Régnault's work on the specific heats of gases this subject has not, so far as I am aware, been experimentally dealt with. The apparatus required to carry out such investigations as those of Régnault is not usually at the command of many physicists.

I have sought to discover methods which would enable the experimenter to prosecute with exactness, yet with less complicated appliances, this inquiry, which has such important bearings upon the mechanical theory of heat and also upon molecular chemistry.

In the following first communication the methods of observation will be described, measurements of the specific heat of air will then be given, and, lastly, the relation between specific heat and temperature in the case of a few gases will be investigated †.

In performing his measurements Régnault used an apparatus which consisted essentially of three parts—the vessel for storing the gas (gasometer), the heating-apparatus, and the calorimeter. Régnault's gasometer was of copper foil 5 millims. in thickness, and had a capacity of about 30 litres; the gas to be examined was compressed in it by means of a force-pump. Pressures varying from 3000 to 4000 millims. of

\* From Pogg. *Ann. der Physik und Chemie*, vol. clvii. pp. 1–42. Translated by M. M. Pattison Muir, The Owens College, Manchester.

† It need hardly be mentioned that Régnault has himself fully discussed the sources of error which may arise in his own methods of observation.

mercury (at  $0^{\circ}$  C.) were frequently employed ; 9000 millims. was an attainable pressure. The gases could easily be sent through the warming-apparatus and calorimeter in sufficient quantity and with the necessary velocity. As the volume of the copper containing-vessel was known, the quantity of gas used could be calculated from the pressure existing before and after the experiment. That part of the apparatus in which the gases were warmed consisted of an oil-bath containing a tube of brass or platinum 8 millims. in width and 10 metres in length, through which the gas was allowed to flow. The calorimeter consisted of a similarly shaped spiral of thin sheet-tin surrounded with water. In taking an observation a known quantity of the gas under examination was allowed to flow through the warming-apparatus, which was heated to a determinate temperature ; the heat thus absorbed was again given up to the water in the calorimeter ; and from a determination of this quantity of heat the specific heat of the gas could be readily calculated.

In order to ensure correct determinations, it was especially necessary that the gas should be completely heated to the temperature of the warming-apparatus, and that it should also entirely give up its heat in the calorimeter. The spiral through which the gas flowed was therefore required to present a considerable surface to the gas and thus to be of considerable length. This was the more requisite, inasmuch as the gas passed through the apparatus with a very considerable velocity, as much as 24 litres per minute. The length of the spiral necessitated that the vessel containing it should be of somewhat large dimensions. In the greater number of Régnault's researches the mass of water used amounted to 660 grms., calculated to the water-value of the spiral, the outer vessel, and the stirrer. In order to bring about a sufficient elevation of temperature in this mass of liquid, a large quantity of heated gas was necessarily employed.

The first problem to be solved appeared then to be how to diminish the size of the calorimeter and at the same time to allow the gas to give up all its heat—in other words, how to bring the heated gas into contact with the largest possible surface in the smallest possible space. For this purpose a method first made use of by Ericson in the construction of his air-machine was employed. It consisted in passing the gas over copper gauze, which exposed a large surface for carrying away the heat.

*Description of the Apparatus (Plate I.).*

The heating-vessel consists of a copper box (hard-soldered), M, 20 centims. in height, 18 centims. in breadth, and 21 cen-

tims. in length, furnished with a funnel-shaped protuberance, P. In this box is placed the thin-walled copper cylinder, G, which has a length of 11 centims. and a diameter of 4 centims., and is filled with fine copper gauze. The gas enters by *mn* and departs by *op*. The latter tube is made of copper so far as *s*, from *s* to *p* it consists of German silver; it is 17 millims. in length. It was found that too much heat was conducted from the warming-vessel to the calorimeter if the exit-tube was made entirely of copper; the correction-constant, *k* (to be discussed hereafter), received therefore too high a value. A thermometer passes through the lid of the heating-vessel. The liquid in the warming-vessel is constantly kept in motion by means of a stirrer, which consists of two perforated horizontal plates hard-soldered to a stirrup V, and at a distance from one another somewhat greater than the breadth of the copper cylinder. A cord passing from V over a pulley is connected with clockwork, whereby the stirrer is raised twenty times per minute; it falls again by its own weight. By an arrangement of sockets any liquid which might be carried up with the stirrer finds its way back into the warming-vessel. The temperature of the water (or paraffin) was regulated so that it did not vary more than, at the utmost,  $1^{\circ}$  during the experiments.

The calorimeter (*k*) consists of a silver vessel, outwardly of a cylindrical shape,  $5\frac{1}{2}$  centims. in height and 42 millims. in width, the thickness of the walls being 0.35 millim. Three vertical silver tubes, bound together by little horizontal tubes, are fused into the calorimeter; these serve for receiving the heat given up by the gas under examination. The gas enters through the tube  $\beta$  placed horizontally 8.5 millims. from the bottom of the cylinder (this tube is 8 millims. in breadth); thence it passes to the first vertical tube, from the upper end of which it is conveyed to the next tube, and so on, finally making its exit by the narrow tube ( $2\frac{1}{2}$  millims. wide) *q*. The individual tubes are 9 millims. in breadth and 41 millims. in height; they are all filled with silver gauze. Through a lid placed on the top of the vessel pass a thermometer, the stirrer, and the tube *q*. The bulb of the thermometer is placed between the first and third tube, the stirrer between the second and third.

Fig. A represents a section of the calorimeter at the level of the entering tube.  $\beta$  is the tube by which the gas enters; *a*, *b*, and *c* are the first, second, and third silver tubes respectively; *t* is the thermometer, and *r* is the stirrer. A small quantity of grease is introduced between the rim of the lid and the vessel to prevent the escape of the water which may be raised

up in stirring. The warming-vessel and calorimeter are connected together by means of a cork, as was done by Régnault in his apparatus.

In order to maintain a constant temperature around the calorimeter during an experiment, the calorimeter is placed in a cylindrical vessel of sheet brass having two walls; the height of this vessel is  $17\frac{1}{2}$  centims., and the diameter  $14\frac{1}{2}$  centims. A funnel-shaped orifice in the front part of this vessel allows of the introduction of the tube  $\beta$  without contact between the two. By means of the side tubes water, maintained at uniform temperature, is allowed to circulate through this vessel. A wooden partition covered with tinfoil,  $z$ , and furnished with a circular opening in the centre, protects the vessel just described from the heat radiated from the warming-apparatus. The calorimeter is supported in a wicker stand in the centre of the brass vessel  $\chi$ . A board covered with tinfoil is placed on the top of  $\chi$ ; and to this board another, protected in the same manner, is vertically attached.

The latter carries a pulley, over which passes the cord attached to the stirrer of the calorimeter; the stirrer is moved upwards and downwards 40 to 50 times per minute.

The thermometer of the calorimeter is illuminated from behind by means of a gas-lamp, the heat-rays from which are cut off by the interposition of four glass plates. Immediately behind the thermometer is placed a glass plate covered, with the exception of a space equal to the breadth of the thermometer, with tinfoil; by means of this arrangement foreign light is cut off.

The thermometer employed was graduated to tenths of a degree; by using a telescope it could be read to hundredths of a degree. This thermometer was calibrated and carefully compared with a standard from  $0^{\circ}$  to  $40^{\circ}$ .

On account of the size of the apparatus and the great pressures required, Régnault's method of storing and delivering the gases, and of measuring their volumes, could not be employed. It was, however, found possible to modify the method of Delaroche and Bérard so as to obtain exact results. These experimenters forced the gases from moist animal bladders, which were placed in a balloon communicating with another into which water issued at a constant velocity. From the quantity of water entering, the quantity of gas issuing could be calculated. By replacing the animal membrane by caoutchouc, the gases could be maintained free from moisture.

The apparatus which I employ has the following disposition:—A large glass balloon (C) is closed air-tight by means



of a caoutchouc stopper. Two glass tubes, a wider ( $\psi$ ) and a narrower ( $g$ ), pass through this stopper. A balloon of thick black caoutchouc,  $Q$ , holding about 20 litres of gas, is connected with the wider tube  $\psi$ , which is closed by means of a caoutchouc stopper carrying two tubes— $i$  leading to the heating-apparatus, and the other, leading to the manometer,  $k$ . The manometer consists of a glass,  $r$ , into which the graduated tube  $k$  is fastened air-tight. By means of the stopcock  $h$ , communication can be made or broken between  $Q$  and the manometer. Very wide stopcocks,  $T$  and  $V$ , are placed on the tubes  $g$  and  $i$ .

The second balloon,  $B$ , besides communicating with  $Q$ , is fitted with two holes—one leading to the manometer,  $e$ , the other passing to the bottom of the vessel, and capable of being brought into connexion by means of the stopcock  $S$  with the water-cistern  $A$ . The tubes establishing communication between  $B$  and  $C$  are so wide that the pressure in each will not alter during the escape of gas from  $Q$ .

The manometers  $e$  and  $k$  contain water; the smallest variations in pressure can therefore be easily read off without the use of a cathetometer. Inasmuch as communication between  $Q$  and  $r$  is made by means of a tube 40 centims. in length and bent many times, and also inasmuch as each experiment only lasts for a short time, the wetting of the gas in the balloon  $Q$  is not to be feared.

$B$  stands upon a bridge-balance, by means of which the weight of this balloon can be ascertained to within 5 grms. with certainty. The gas escaping from  $Q$  is sent through an apparatus  $F$ , consisting of two tubes so connected together that the gas passes through the inner, then gives up its heat to a thermometer, and finally passes on through the outer tube. These tubes are placed in a wooden case. The temperature possessed by the gas issuing from  $Q$  is thus ascertained. By causing the gas to bubble through a cylinder filled with sulphuric acid or with mercury,  $x$ , the rate at which it is flowing is noticed. The apparatus  $y$  is filled with little glass beads, and serves to retain any sulphuric acid which may have been mechanically carried over from  $x$ .

Before carrying out determinations of the specific heats of gases with the apparatus just described, it was necessary to ascertain with certainty whether or not the temperature of the gas as it issued from the heating-vessel was really the same as that of the liquid in the vessel. For this purpose an apparatus similar to that employed for determining the temperature of the gas before it entered the heating-vessel was placed in connexion with the German-silver tube leading

from the heating-vessel. When 10 litres of gas were passing through the apparatus per minute the thermometer remained constant at  $100^{\circ}$ .

Another method for determining that the gases are thoroughly warmed has been proposed by Régnault. He placed a thermometer in the anterior part of the spiral of the heating-vessel. This method is open to the objection that the thermometer is exposed to the influence of the heat radiated from the sides of the apparatus. A gas which has only cooled slightly will therefore produce an extremely small effect on the indication of the thermometer.

In order to ascertain whether the heated gas entirely parted with its heat in the calorimeter, experiments were carried out with a similarly constructed instrument, in which the three tubes filled with silver were replaced by two tubes filled with copper; the numbers representing the specific heats were identical within the limits of experimental error. It was also determined that the heat was entirely given up during the passage of the gas through the first tube; when one of the tubes only was filled with copper gauze and a thermometer surrounded with gauze was placed in the second tube, the temperature indicated by the thermometer was almost identical with that of the surrounding liquid. By using three tubes no doubt could arise as to whether the heat was entirely given up in the calorimeter.

Even if the gas did not enter and leave the calorimeter at the exact temperature indicated by the thermometer in the heating-vessel and in the calorimeter respectively, the result of the experiments would be but very slightly affected thereby, inasmuch as the total cooling of the gas is represented by the difference between these two temperatures, and this difference always amounts to at least  $75^{\circ}$  C.

#### *Method of Observation.*

In carrying out the measurements the following was the method of procedure:—

The balloon Q, after having been entirely emptied, was filled with the gas to be examined, the gas being pure and dry.

For this purpose the balloons B and C were disconnected, and the stopcocks T and V were opened. In order that the gas should enter Q at a fixed temperature, it was caused to traverse a cylinder filled with copper gauze and surrounded with water, which was maintained at the temperature of the air of the room.

The thermometer (F) was arranged so as to indicate the

temperature of the gas as it entered the balloon. The time allowed to elapse between the filling of Q and the carrying-out of the experiment never exceeded half an hour ; in this short time the temperature of the surroundings and of the gas itself altered but slightly ; the thermometer F indicated very nearly the true temperature of the gas in the balloon as that gas for the second time passed over its surface.

When a new gas was to be examined, the balloon Q, having been emptied, was only partially filled with this gas and again emptied before being finally filled ; in this way the last traces of the gas originally in the balloon were removed.

In filling Q the gas was allowed to stream through until the walls of the balloon were stiffly distended. On account of their weight the walls of the balloon then pressed upon the contained gas with a force equal to 1 centim. of water, as determined by the manometer *k*.

The stopcock V being closed the calorimeter was connected in its place. B and C were connected and the pinchcock E was removed ; *b* and *c* in the balloon B were then connected, D was removed, and water was allowed to flow from A until the manometers *k* and *e* showed the pressure at which the gas was to be allowed to issue from Q.

S having been then closed, D and E replaced, and A and B and B and C disconnected, the balloon was weighed in the balance W. A, B, and C were then once more connected, D and E were again removed, and T was opened ; S and V remained meanwhile closed.

The temperature of the calorimeter was noticed as the apparatus was placed in position. The telescope, through which the temperature of the calorimeter was read off, was so placed that one could look through it and at the same time open the stopcocks V and S with the hand.

The position of the thermometer in the calorimeter was read off from minute to minute. Five or ten minutes after the beginning of the experiment V and S were opened at the same moment (marked by means of a second stop-watch), and the gas was allowed to flow through the heating-vessel and calorimeter. By carefully regulating the stopcock S the manometers *k* and *e* could be readily maintained constant ; the gas in Q was therefore maintained at the same pressure throughout the experiment, and consequently flowed through the calorimeter at a constant rate. The temperature of the calorimeter was determined each minute as the gas passed through it. So soon as the rise of temperature had attained to a certain amount (between  $5^{\circ}$  and  $12^{\circ}$ ), the flows of gas and of water into B were simultaneously checked by suddenly

closing V and S, and the watch was read off. Then the temperatures were noted for five to ten minutes—at first each second, afterwards from minute to minute. A regular increase of temperature occurred generally at the tenth and twentieth seconds after closing the stopcocks; so short was the time required for the temperature of the silver gauze to become equalized with that of the water of the calorimeter. The time was longer the higher the temperature of the entering gas, inasmuch as the gauze situated nearest to the entrance-aperture underwent a greater rise in temperature. Immediately before and after the experiment the temperature of the heating-vessel was determined, the mean of the two readings being taken as the temperature during the experiment.

Water maintained at the temperature of the air of the room flowed continuously through the double-walled vessel  $\chi$ . The temperature of this water was determined before it entered and when it flowed from  $\chi$ . Notwithstanding the wooden screen, a slight increase in the temperature of the water was observed; the mean of the two readings was adopted.

A, B, and C being again disconnected, B was reweighed; it had increased by the amount of water which had entered it.

Inasmuch as temperature and pressure remained constant in B and C during the experiment, the volume of water (calculated from the weight) which had entered B expressed the volume of gas which had flowed from Q. It did not signify whether diffusion had occurred through the caoutchouc balloon or not; if this did occur, it could only somewhat decrease the purity of the gas. It was directly shown by experiment (using carbonic acid and ammonia) that the gas in Q became mixed with but traces of air. Although the water did not possess the temperature of  $4^{\circ}\text{C}$ ., the error arising from this appeared so small as not to necessitate the use of a correction.

In order to calculate the weight of the issuing gas, it was necessary that its temperature and pressure should be known. The former was obtained, as has been already described, by reading off the thermometer F. The latter was composed of two parts—of the atmospheric pressure, and of the pressure indicated by  $k$ . If the barometer-reading reduced to  $0^{\circ}$  be represented by B, the reading of the manometer  $k$  reduced to mercury be  $p$ , the temperature of the gas be  $x$ , and the observed volume of the gas be  $v$ , then the calculated volume at  $0^{\circ}$  and 760 millims. pressure is

$$v_0 = \frac{p + B}{760} \cdot \frac{273 \cdot 2}{273 \cdot 2 + x} \cdot v.$$

If  $\sigma$  represent the specific gravity of the gas referred to air.

then, as the specific gravity of air at  $0^{\circ}$  and 760 millims. referred to water is  $\frac{1}{773.3}$ , the weight of volume  $v$  of the gas is

$$G = \frac{v_0 \sigma \cdot 1}{773.3} = \frac{p+B}{760} \cdot \frac{273.3 \cdot \sigma}{(273.3+x)} \cdot \frac{v}{773.3}$$

Lastly, if  $W$  represents the weight in kilos of water which entered the balloon during the experiment, and if the litre be taken as unit of volume, then

$$G = \frac{p+B}{760} \cdot \frac{273.3 \cdot \sigma}{(273.3+x)} \cdot \frac{W}{773.3}$$

In order to calculate the specific heat, we require also the weight of liquid in the calorimeter, as also the water-value of the calorimeter. The weight of liquid was determined by direct weighing before every experiment; it varied from 59 to 61 grms. The water-value of the calorimeter was calculated. The specific heat of the silver used for making the vessel was 0.0607; its weight amounted to 80.122 grms.; the water-value was therefore 5.07. The water-value of that part of the thermometer which was situated in the calorimeter, and which was measured, amounted to 0.3944. The total water-value of the calorimeter was therefore 5.4644. A small error in the estimation of the water-value of the thermometer would not greatly influence the final result, because of the smallness of the total figure.

The rise of temperature observed by means of the thermometer in the calorimeter must not be attributed solely to the heat given up by the gas, and cannot be directly used for calculating the specific heat of the gas; but a correction must be applied, partly because of heat conducted through the German-silver tube from the heating-vessel, and partly because of heat radiated to or from the calorimeter.

By passing air at the same velocity as was done with the various gases through the calorimeter (surrounded with water at the temperature of the air), it was proved that no appreciable amount of heat is generated by friction of the particles of gas against the silver gauze in the calorimeter.

We assume that the amount of heat gained by the calorimeter by conduction is constant for each unit of time during the whole experiment\*; and we designate the rise of temperature per minute caused by this heating of the calorimeter by  $k$ .

\* Régnault, *Relation des expériences pour déterminer les lois et les données physiques nécessaires au calcul des Machines-à-feu*, p. 79 et seq.

In order to estimate the heat radiated to and from the calorimeter, as also that carried away by currents of air, we make the assumption (justified by the smallness of the differences of temperature) that this is proportional to the difference of temperature between the calorimeter and its surroundings. We shall designate the rise of temperature per minute brought about by radiation &c., for a temperature-difference of  $1^{\circ}$  between calorimeter and surroundings, by  $\alpha$ . The measurements taken before and after the gas was allowed to flow through the apparatus serve as data for calculating the values of  $\alpha$  and  $k$ .

Let  $T$  represent the mean temperature of the calorimeter during the initial period,  $T_1$  the mean temperature during the final period; let  $\tau$  represent the temperature of the surroundings;  $m$  and  $m_1$  the increase of temperature of the calorimeter per minute during these periods; then

$$-(T - \tau)\alpha + k = m,$$

$$-(T_1 - \tau)\alpha + k = m_1;$$

hence we deduce

$$\alpha = \frac{m - m_1}{T_1 - T}$$

and

$$k = m + \frac{(T - \tau)(m - m_1)}{T_1 - T}.$$

It may readily be shown that it is not really necessary to know the temperature of the surroundings (that is, the value of  $\tau$ ), inasmuch as it remains constant throughout the experiment.

For this purpose let us suppose that the rise of temperature is uniform so long as gas passes through the calorimeter (and with a constant flow of gas this is almost exactly true), and let us calculate, on this supposition, the mean correction which it is necessary to apply to the observed temperature for each minute of time.

Let the temperature of the calorimeter at the moment when the gas begins to pass through be represented by  $t_1$ , and at the moment of closing the stopcock by  $t_2$ . Then the mean temperature during the experiment is

$$\frac{t_1 + t_2}{2}.$$

The correction is then

$$-\alpha \left\{ \frac{t_1 + t_2}{2} - \tau \right\} + k;$$

and by substituting the values of  $\alpha$  and  $k$  we get

$$-\frac{m-m_1}{T_1-\tau} \left\{ \frac{t_1+t_2}{2} - \tau \right\} + m + \frac{(T-\tau)(m-m_1)}{T_1-T} = C,$$

$$\frac{m-m_1}{T_1-T} \tau + m - \frac{m-m_1}{T_1-T} \frac{t_1+t_2}{2} + \frac{T(m-m_1)}{T_1-T} - \tau \frac{m-m_1}{T_1-T} = C,$$

an equation in which the two terms containing  $\tau$  cancel one another. The correction is therefore independent of the temperature of the surroundings,  $\tau$ .

The correction necessitated by the fact that the whole of the mercury of the thermometer was not surrounded by liquid must, of course, be applied to the temperatures  $t$  and  $t_1$ .

The temperature indicated by the thermometer ten seconds after closing the stopcocks was taken to be  $t_1$ : a part at least of the increase of temperature noticed during this short period must be traced to the fact that the thermometer and its surroundings, as also the silver gauze, did not possess the same temperature as the liquid of the calorimeter. Nevertheless the result is unaffected, whether we choose for  $t_1$  this temperature or that indicated at the moment of closing the stopcocks.

We must now estimate the cooling of the gases. If  $M$  represent the constant (mean) temperature of the heating-vessel during the experiment, then the cooling is represented by

$$\Theta = M - \frac{t_1 + t_2}{2}.$$

But if  $W$  be the weight of water in the calorimeter added to the water-value of the same,  $G$  the weight of the gas,  $\theta_1$  the corrected temperature-increase of the calorimeter, and  $C$  the specific heat of the gas under examination, then

$$W \cdot \theta_1 = cG\Theta,$$

also

$$c = \frac{W\theta_1}{G\Theta}.$$

By multiplying the specific heats by the densities of the gases, referred to air, we obtain the specific heats of equal volumes of the different gases, if that of air be taken, for unit volume, as equal to 0.2389; that is, we obtain so-called relative heats.

In order to determine the influence of temperature upon the specific heats of the different gases, they were heated once to  $100^\circ$  and another time to  $200^\circ$ , and the mean specific heat estimated between this temperature  $t$  and the temperature  $\tau$  ( $20-25^\circ$ ) of the liquid in the calorimeter. If we represent by  $Q$  the amount of heat which must be given up to the gas in order to raise its temperature from  $\tau^\circ$  to  $t^\circ$ ,  $Q$  may be repre-

sented as a function of the temperature by the equation

$$Q = a(t - \tau) + b(t - \tau)^2,$$

where  $a$  and  $b$  are constants.

If, however,  $c_1$  is the mean specific heat between  $\tau^\circ$  and  $100^\circ$ ,  $c_2$  that between  $\tau^\circ$  and  $200^\circ$ , then  $t = 100$  in one case, and in the other  $= 200$ , and then  $Q$  is equal to the mean specific heat  $c$  multiplied by the temperature-increase; so we have

$$Q = c(t - \tau),$$

$$c_1 = (100 - \tau) = a(100 - \tau) + b(100 - \tau)^2,$$

$$c_2 = (200 - \tau) = a(200 - \tau) + b(200 - \tau)^2,$$

or

$$c_1 = a + b(100 - \tau),$$

$$c_2 = a + b(200 - \tau),$$

equations from which  $a$  and  $b$  may be immediately deduced. But the specific heat is the change in the quantity of heat contained in unit weight of the body for unit of temperature, *i. e.*

$$\frac{dQ}{dt}.$$

For the true specific heat at temperature  $t$  it follows that

$$C = a + 2b(t - \tau);$$

from which we can directly deduce the relative heats.

In the following Tables the observed and calculated results of the determinations of specific heats of various gases made in accordance with the methods described above are recorded. In those Tables,—

W represents the weight of water in the calorimeter;

G the weight, in kilogrammes, of water entering the balloon, and the volume, in litres, of the gas flowing from the balloon;

B the barometric pressure;

$p$  the pressure read off on the manometer  $k$ ;

$\chi$  the temperature of the gas in the caoutchouc balloon;

$n$  the duration of the experiment, in minutes;

$a$  the volume of gas passing from the balloon per minute;

$\tau$  the temperature of the surroundings;

T the mean temperature of the calorimeter during the initial period;

$m$  the rise of temperature of the calorimeter in each minute of the initial period;

$T_1$  the mean temperature of the calorimeter during the final period;

$m_1$  the rise of temperature of the calorimeter in each minute of the final period;



$k$  and  $\alpha$  the constants discussed on p. 90 ;  
 $\theta$  the observed temperature-increase of the calorimeter ;  
 $\theta_1$  the corrected temperature-increase of the calorimeter ;  
 $t$  the mean temperature of the calorimeter ;  
 $M$  the temperature of the heating-vessel ;  
 $\Theta$  the temperature-decrease of the gas ;  
 $c$  the specific heat of the gas.

*Air.*

The air coming from a gasometer was dried by passing through sulphuric acid in a Woulfe's bottle, and through a cylinder containing beads soaked in sulphuric acid.

	1.	2.	3.	4.	5.	6.
W .....	58.8	60.68	59.67	59.96	61	58.41
G .....	15.56	20.32	17.67	20.13	17.43	17.76
B .....	759.9	769	760.3	760.3	758.7	755.26
$p$ .....	75.08	62	60.80	60.4	56.70	71.40
$\chi$ .....	15.6	16	16.8	17.3	16.4	16.4
$n$ .....	$3\frac{1}{2}$	5	$4\frac{1}{2}$	5	$4\frac{1}{2}$	4
$a$ .....	4.45	4.06	3.93	4.03	3.87	4.44
$\tau$ .....	15.8	16.2	17.2	17.2	16.8	16.8
T .....	14.46	15.22	16.38	16.85	15.61	16.23
$m$ .....	0.0604	0.064	0.088	0.06	0.072	0.0675
$T_1$ .....	21.07	23.26	23.71	24.635	22.60	23.51
$m_1$ .....	-0.013	-0.03	0.00	-0.032	0.0025	-0.026
$k$ .....	0.0486	0.0526	0.0780	0.056	0.058	0.059
$\alpha$ .....	0.0117	0.0117	0.012	0.0118	0.011	0.013
$\theta$ .....	6.48	7.95	7.1	7.8	6.75	7.13
$\theta_1$ .....	6.39	7.85	6.9	7.74	6.61	7.06
$t$ .....	17.86	19.42	20.15	20.9	19.22	20.06
M .....	100°	100°	100°	100°	100°	99.8
$\Theta$ .....	82.14	80.58	79.85	79.1	80.78	79.74
$c$ .....	0.2389	0.2395	0.2413	0.2414	0.2376	0.2398

	7.	8.	9.	10.	11.	12.
W .....	60.68	60.25	60.33	61.53	60.62	60.04
G .....	18.22	16.55	16.24	17.66	18	17.45
B .....	755.26	755.26	751.62	751.1	745.7	745.7
$p$ .....	55.08	84.4	76.30	81.19	76.1	76.1
$\chi$ .....	16.8	16.8	17.00	17.8	21.5	22.2
$n$ .....	5	$3\frac{1}{2}$	$3\frac{2}{3}$	$3\frac{2}{3}$	$3\frac{2}{3}$	$3\frac{1}{2}$
$a$ .....	3.64	4.73	4.42	4.82	4.7	4.65
$\tau$ .....	17	17	16.6	17.5	21.2	22.2
T .....	16.72	17.08	15.81	16.67	20	19.63
$m$ .....	0.073	0.071	0.083	0.08	0.0857	0.1
$T_1$ .....	23.82	23.89	22.53	23.7	26.86	26.65
$m_1$ .....	-0.01	-0.006	-0.003	-0.003	-0.02	+0.021
$k$ .....	0.0702	0.071	0.072	0.0695	0.067	0.0708
$\alpha$ .....	0.0118	0.0112	0.0128	0.0117	0.0154	0.0112
$\theta$ .....	6.93	6.53	6.46	6.86	6.62	6.71
$\theta_1$ .....	6.78	6.42	6.32	6.72	6.50	6.49
$t$ .....	20.40	20.63	19.33	20.30	23.6	23.24
M .....	99.8	99.8	99.7	99.7	100.9	102
$\Theta$ .....	79.4	79.17	80.37	79.4	77.3	78.76
$c$ .....	0.2379	0.2385	0.2395	0.2407	0.2374	0.2384

The mean value is 0·2389, the greatest deviation from this being 0·0025.

Experiments Nos. 9 and 10 were carried out four weeks after Nos. 1 to 8, and Nos 11 and 12 some time after 9 and 10 ; during the earlier experiments the heating-vessel contained boiling water ; during the two latter experiments it contained melted paraffin.

The numbers agree among themselves to as great an extent as do those obtained by Régnault : thus he obtained numbers varying from 0·23536 to 0·23890 ; my numbers vary from 0·2374 to 0·2414. The mean number 0·2389 obtained by me differs by less than 1 per cent. from that obtained by Régnault, viz. 0·23751.

The method adopted for graduating the thermometer for 150° and 200° may fitly be described in connexion with the specific heat of air.

In order to determine the temperatures which corresponded, in an air-thermometer, to the 150th and 200th degree of the thermometer used in the heating-vessel, the paraffin-bath was heated steady until the mercury column reached and remained steadfast at the given degree. The process, as if for determining the specific heat of air, was then proceeded with ; and the lowering of temperature of the air of the calorimeter was calculated from the known specific heat of air (which, as shown by Régnault, is independent of temperature) and from the other data of the experiment.

By adding this lowering of temperature to the mean temperature of the liquid in the calorimeter during the experiment, which corresponds with the final temperature of the air after cooling, the initial temperature of the air was obtained, and therewith also the temperature measured by an air-thermometer which corresponded with the position of the thermometer. The differences of the calculated deviations of our thermometers from an air-thermometer are of the same order as those occurring in the estimation of specific heat itself.

### *Hydrogen.*

The hydrogen was prepared from zinc and dilute sulphuric acid in a Kipp's machine, purified by passing through caustic potash, and dried by means of sulphuric acid. The following Table expresses the mean specific heats obtained for hydrogen between 100° and 20°.

	1.	2.	3.	4.	5.	6.
W .....	60·43	60·15	60·73	60·10	59·9	60·18
G .....	19·10	16·70	17·95	17·68	18·42	17·77
B .....	751·09	751·09	755·22	755·10	759·8	759·8
$p$ .....	26·70	26·52	19·18	25·10	26·38	20·4
$\chi$ .....	17·80	18·20	18·20	18·90	17·80	18·6
$n$ .....	$2\frac{5}{8}$	$2\frac{1}{2}$	$3\frac{1}{2}$	$2\frac{3}{4}$	$3\frac{1}{2}$	4
$\tau$ .....	17·4	17·80	18·00	18·7	17·1	18·2
$a$ .....	5·77	6·70	5·13	6·43	5·26	4·44
$T$ .....	16·80	18·00	18·95	18·22	17·5	16·60
$m$ .....	0·10	0·10	0·083	0·104	0·057	0·08
$T_1$ .....	24·00	24·54	25·44	24·89	24·60	24·01
$m_1$ .....	0·04	0·05	0·02	0·022	—0·018	—0·004
$k$ .....	0·095	0·1084	0·092	0·0982	0·066	0·0617
$\alpha$ .....	0·0083	0·00764	0·0097	0·0123	0·0197	0·0114
$\theta$ .....	6·80	6·005	6·20	6·3	6·7	6·52
$\theta_1$ .....	6·62	5·82	6·02	6·134	6·632	6·38
$t$ .....	20·40	21·30	22·30	21·63	21·05	20·14
$M$ .....	99·70	99·70	99·80	99·80	100	100
$\Theta$ .....	79·30	78·40	77·50	78·17	78·95	79·86
$c$ .....	3·336	3·384	3·333	3·371	3·413	3·406

The mean number deduced from experiments 5 and 6 is 3·410, the greatest deviation therefrom being 0·004. The fact that the first four numbers are larger than the last two is explained by Régnault's observation that the constant  $k$  acquires a larger value when the whole apparatus is filled with hydrogen (as was the case in the first four experiments) than when it is filled with air. When the very mobile hydrogen is allowed to remain at rest before and after the experiments, currents are engendered which carry over heat from the heating-vessel to the calorimeter, without taking into account the heat conducted from the German-silver tube. But in making the correction, the heat conducted over is alone taken into account, when the apparatus is filled with a stream of gas, as the secondary currents are then entirely neutralized. But too great a value for  $k$  causes the negative correction for the observed temperature-increase to be also too large, and therefore the temperature-increase itself to be too small. The smallness of the number obtained for the specific heat is thus accounted for.

In the last two experiments the apparatus was filled with air before and after each experiment;  $k$  has in these cases the smaller and more correct value, and the number 3·410 then obtained for the specific heat of hydrogen agrees well with Régnault's number 3·409, especially when it is remembered that his numbers vary from 3·401 to 3·419.

By multiplying 0·9692 by 3·410 the number 0·2358 is obtained as representing the specific heat referred to volume.

It did not appear to be of interest to determine the specific

heat of hydrogen at varying temperatures, inasmuch as Régnault has shown that in the case of this gas the specific heat is independent of the temperature. Neither did an examination of oxygen or of nitrogen appear to be necessary. Régnault has shown that the specific heat of the first, referred to volume, is the same as that of air; the same must therefore hold good for nitrogen. Inasmuch also as air exhibits no change of specific heat with change of temperature, we must believe that neither is there any change in the case of its constituents.

### Carbonic Acid.

The carbonic acid was generated in a Kipp's apparatus from marble and dilute hydrochloric acid. It was purified from traces of hydrochloric acid by passing through a solution of sodium bicarbonate, and was then dried by means of sulphuric acid. The carbonic acid employed never contained more than from  $\frac{1}{2}$  to 1 per cent. of air when examined after coming from the balloon.

The following Table contains the results of the experiments.

TABLE I.—Experiments between  $100^{\circ}$  and  $20^{\circ}$  to  $25^{\circ}$ .

	1.	2.	3.	4.	5.	6.	7.	8.	9.
W...	59.84	59.15	60.63	60.63	60.70	60.85	59.82	59.74	60.04
G...	15.13	15.12	17.51	14.17	13.72	16.12	16.83	16.10	17.27
B...	755.90	755.43	755.60	755.7	755.72	746.9	746.9	745.3	745.36
p...	17.70	16.60	17.2	17.4	18.3	20.5	21.2	22.2	20.5
X...	86.10	66.50	75.48	82.82	56.83	77.5	78.74	61.61	69.36
n...	$4\frac{1}{2}$	4	$4\frac{1}{3}$	$3\frac{1}{2}$	4	4	4	$4\frac{1}{2}$	$4\frac{1}{2}$
a...	3.36	3.78	4.04	4.22	3.43	4.03	4.16	3.58	3.84
r...	18.00	16.70	17.0	17.4	17.8	20.45	4.20	21.9	22.8
T...	17.10	17.1	16.62	16.3	17.3	20.52	18.42	17.51	21.7
m...	0.075	0.07	0.079	0.075	0.073	0.043	0.075	0.011	0.067
T <sub>1</sub> ...	25.24	25.06	25.80	24.25	24.46	27.95	26.93	26	29.62
m <sub>1</sub> ...	-0.023	-0.03	-0.04	-0.024	-0.013	-0.06	-0.023	0.00	-0.04
k...	0.0642	0.076	0.085	0.0616	0.0669	0.044	0.0429	0.0563	0.0521
α...	0.0121	0.0127	0.014	0.0125	0.0120	0.0139	0.0115	0.013	0.0135
θ...	7.92	7.82	9.0	7.72	6.97	7.49	8.35	8.10	7.84
θ <sub>1</sub> ...	7.81	7.74	8.89	7.64	6.85	7.53	8.35	7.86	7.79
t...	21.36	21.22	21.40	20.46	21.00	24.37	22.82	21.95	25.8
M...	99.80	99.80	99.80	99.80	99.80	98.60	100.25	101.25	97.5
Θ...	78.44	78.58	78.40	79.34	78.80	74.23	77.43	79.3	71.7
c...	0.2084	0.2081	0.2101	0.2095	0.2113	0.2086	0.2091	0.2061	0.2082

The mean value is 0.2088; the greatest deviation from this is 0.0027. Experiments 1-5 were carried out about a month before experiments 6-9. In the first experiments the heating-vessel was filled with boiling water, in the latter with melted paraffin.

TABLE II.—Experiments between 150° and 25°.

	1.	2.	3.	4.	5.
W .....	60.45	60.65	60.37	60.35	60.8
G .....	16.40	12.43	12.54	12.58	13.5
B .....	747.6	747.1	747.1	745.2	745.2
<i>p</i> .....	67.73	64.87	65.7	53.0	57.5
$\chi$ .....	19.6	20.4	20.9	19.4	20.6
<i>n</i> .....	4½	3½	3½	4½	4
<i>a</i> .....	3.64	3.55	3.58	3.025	3.38
$\tau$ .....	19.6	20.6	21.5	19.3	20.35
T .....	19.55	20.85	21.82	19.36	19.35
<i>m</i> .....	0.114	0.11	0.116	0.103	0.1
T <sub>1</sub> .....	33.20	31.55	32.05	29.9	29.94
<i>m</i> <sub>1</sub> .....	-0.046	-0.0125	-0.035	-0.02	-0.017
<i>k</i> .....	0.114	0.1113	0.121	0.1036	0.089
<i>a</i> .....	0.0117	0.0114	0.0148	0.0116	0.0109
$\theta$ .....	13.43	10.42	10.08	10.28	10.4
$\theta_1$ .....	13.3	10.26	9.94	10.12	10.24
<i>t</i> .....	26.7	26.4	27.10	24.86	24.8
M .....	151.5	155.1	150.7	151.7	146.0
$\Theta$ .....	124.8	128.7	123.6	126.8	121.15
<i>c</i> .....	0.2160	0.2170	0.2143	0.2147	0.2138

The mean value is 0.2152, and the greatest deviation therefrom 0.0019.

TABLE III.—Experiments between 200° and 25°.

	1.	2.	3.	4.	5.	6.
W .....	60.4	60.52	60.7	60.3	60.7	59.75
G .....	8.36	9.11	10.01	8.4	10.05	10.12
B .....	745.2	744.63	744.65	744.65	749.0	749.00
<i>p</i> .....	46.1	47.3	61.2	65.3	62.83	61.20
$\chi$ .....	21.8	22.8	23.4	23.1	20.00	20.1
<i>n</i> .....	3	3½	3½	2½	3	3½
<i>a</i> .....	2.786	2.88	3.16	3.36	3.35	3.20
$\tau$ .....	22	23.25	23.7	23.8	20.2	20.7
T .....	21.72	21.67	18.6	21.8	19.1	19.76
<i>m</i> .....	0.14	0.145	0.20	0.20	0.170	0.151
T <sub>1</sub> .....	31.99	32.55	31.16	32.45	31.30	31.93
<i>m</i> <sub>1</sub> .....	0.038	0.039	0.06	0.05	0.04	0.007
<i>k</i> .....	0.137	0.129	0.1435	0.172	0.158	0.133
<i>a</i> .....	0.01	0.097	0.0112	0.0141	0.0107	0.0118
$\theta$ .....	9.74	10.24	11.72	9.84	11.53	11.70
$\theta_1$ .....	9.485	9.9	11.31	9.53	11.22	11.48
<i>t</i> .....	27	27.2	25.05	27.3	25.35	26.04
M .....	202.1	200.45	201.1	202.8	196.35	196.25
$\Theta$ .....	175.1	173.25	176.05	175.5	171	170.20
<i>c</i> .....	0.2230	0.2182	0.2193	0.2181	0.2188	0.2207

The number 0.2197 represents the mean value, and 0.0033 the greatest deviation from the mean.

My numbers agree among themselves as well as those obtained by Régnault; for instance, he obtained values for the specific heat between  $210^{\circ}$  and  $10^{\circ}$  varying from 0.21416 to 0.21844 &c. It appears, alike from Régnault's and from my experiments, that there is a great change in the specific heat of the gas under examination with change of temperature.

The true specific heats calculated from the foregoing numbers for certain temperatures are as follows:—

For	0	.	.	.	.	.	0.1952
	100	.	.	.	.	.	0.2169
	200	.	.	.	.	.	0.2387

Régnault's numbers are:—

For	0	.	.	.	.	.	0.1870
	100	.	.	.	.	.	0.2145
	200	.	.	.	.	.	0.2397

Taking the specific gravity of carbonic acid as 1.529, the following numbers express the relative heats:—

For	0	.	.	.	.	.	0.2985
	100	.	.	.	.	.	0.3316
	200	.	.	.	.	.	0.3650

#### *Carbonic Oxide.*

The gas was prepared by Fowne's method, viz. heating one part of yellow prussiate of potash with eight parts of concentrated sulphuric acid. The gas was led through caustic potash and then through sulphuric acid: it was entirely absorbed by a solution of cuprous chloride in hydrochloric acid; it contained no carbonic acid. Oxalic acid was not used for the preparation of the gas, inasmuch as the carbonic acid simultaneously produced can only be separated with great difficulty.

The numbers given in the two following Tables yield almost exactly the same values for the specific heat; so that we may conclude that the specific heat of carbonic oxide is independent of the temperature. Régnault's number between  $200^{\circ}$  and  $20^{\circ}$ , viz. 0.2450, does not differ materially from mine.

The relative heat of carbonic oxide is 0.2346 (the specific gravity being taken as 0.967), which is nearly the same as that of air — 0.2389.

TABLE I.—Experiments between 100° and 25°.

	1.	2.	3.	4.	5.
W .....	60.36	60.85	61.03	60.65	60.13
G .....	18.38	16.45	18.31	18.81	18.47
B .....	745.6	745.6	750.5	750.3	753.3
p .....	75.5	73.7	77.9	77.1	80.8
$\chi$ .....	22.8	23.1	19.9	20.4	20.2
$n$ .....	$4\frac{1}{2}$	4	$4\frac{1}{2}$	$4\frac{1}{2}$	$4\frac{1}{2}$
$a$ .....	4.084	4.11	4.07	4.18	4.105
$\tau$ .....	22.6	23	20.05	20.3	19.95
T .....	21.05	18.55	18.07	18.05	20.48
$m$ .....	0.0625	0.1104	0.079	0.083	0.073
$T_1$ .....	27.65	24.93	25.09	25.19	27.68
$m_1$ .....	-0.017	0.027	-0.004	0.0033	-0.013
$k$ .....	0.0442	0.050	0.0557	0.057	0.079
$\alpha$ .....	0.0120	0.0119	0.0117	0.0112	0.0103
$\theta$ .....	6.40	5.98	6.73	6.88	7.00
$\theta_1$ .....	6.3	5.72	6.57	6.69	6.84
$t$ .....	24.5	21.8	21.71	21.74	24.20
M .....	98	97.6	98.6	98	103
$\Theta$ .....	73.5	75.8	76.9	76.26	78.8
$c$ .....	0.2450	0.2420	0.2432	0.2425	0.2399

Mean value = 0.2425 ; greatest deviation = 0.0026.

TABLE II.—Experiments between 200° and 25°.

	1.	2.	3.	4.	5.
W .....	60.95	59.8	60	59.9	60.65
G .....	11.27	11.70	12.71	11.5	12.61
B .....	750.15	750.15	750.15	753.3	753.3
p .....	44.9	58.34	57.52	45.3	65.2
$\chi$ .....	21.6	21.4	21.6	20.4	20.4
$n$ .....	4	$3\frac{1}{2}$	4	$4\frac{1}{2}$	4
$a$ .....	2.82	3.34	3.18	2.55	3.15
$\tau$ .....	22.2	21.7	21.55	20.65	20
T .....	22.6	17.63	21.54	21.15	21
$m$ .....	0.2	0.234	0.184	0.18	0.17
$T_1$ .....	32.9	28.45	32.9	31.35	32.23
$m_1$ .....	0.07	0.05	0.04	0.0571	0.033
$k$ .....	0.205	0.166	0.184	0.182	0.182
$\alpha$ .....	0.0126	0.017	0.0127	0.0121	0.0122
$\theta$ .....	9.92	9.63	10.48	9.33	10.43
$\theta_1$ .....	8.70	9.15	10.08	8.83	10.03
$t$ .....	28	23.38	27.43	26.44	26.8
M .....	202.75	194.75	199.1	194.9	198.45
$\Theta$ .....	174.75	171.38	171.67	168.46	171.65
$c$ .....	0.2407	0.2407	0.2442	0.2444	0.2431

Mean value = 0.2426 ; greatest deviation = 0.0019.

*Ethylene.*

The ethylene was produced by Mitscherlich's method, viz. by leading alcohol vapour into tolerably concentrated sulphuric acid heated to  $160^{\circ}$  or  $170^{\circ}$ . The gas was cooled to condense ether, which is always produced along with the ethylene, and then passed through caustic potash and through concentrated sulphuric acid into a gasometer. The last traces of ether were given up to the water in the gasometer; and the gas, after being dried by means of sulphuric acid, could be passed into the caoutchouc balloon.

Inasmuch as by-products are always formed in the production of ethylene, so great an agreement among the results of experiments cannot be expected in the case of this as of the other gases.

TABLE I.—Experiments between  $100^{\circ}$  and  $25^{\circ}$ .

	1.	2.	3.	4.	5.	6.	7.	8.
W ...	60.4	60.51	60.9	60.88	60.7	60.88	60.73	60.29
G ...	14.95	15.68	15.4	16.1	15.25	16.13	17.72	15.11
B ...	748.7	748.4	748.4	748.4	750.84	750.84	750.84	748.3
p ...	80.37	81.20	82.42	74.26	78.33	78.33	79.15	80.8
$\chi$ ...	20.8	21.8	21.8	21.6	21.8	22.2	23.1	22.9
n ...	3	3	3	$3\frac{1}{2}$	3	$3\frac{3}{8}$	$3\frac{1}{2}$	3
a ...	5.00	5.22	5.1	4.92	5.08	5.34	5.06	5.03
$\tau$ ...	21.20	21.9	22	21.7	21.9	22.2	22.75	25
T ...	21.12	20.23	19.23	20.25	18.55	18.11	20.05	19.84
m ...	0.071	0.073	0.094	0.088	0.1	0.137	0.068	0.115
T <sub>1</sub> ...	29.54	29.14	28.37	29.4	27.15	27.8	30.06	29.19
m <sub>1</sub> ...	-0.056	-0.047	-0.02	-0.057	-0.017	-0.033	-0.06	-0.036
k ...	0.070	0.050	0.059	0.064	0.0554	0.046	0.033	0.0632
$\alpha$ ...	0.015	0.0134	0.0124	0.0158	0.0136	0.0141	0.0128	0.016
$\theta$ ...	8.26	8.74	8.98	9.1	8.3	9.37	9.98	9
$\theta_1$ ...	8.25	8.71	8.88	9.07	8.18	9.27	9.98	8.9
t ...	25.57	24.93	23.96	25.15	23	23.21	25.30	24.75
M ...	100.65	100	101	102	97	100.5	100	102.1
$\Theta$ ...	75.1	75.07	77.1	76.85	74	77.29	74.7	77.35
c ...	0.3803	0.3852	0.3912	0.3867	0.3783	0.3896	0.3950	0.3973

Mean value = 0.3880 ; greatest deviation = 0.0093.



TABLE II.—Experiments between 200° and 27°.

	1.	2.	3.	4.	5.	6.
W .....	60·81	59·85	60·65	60·43	60·75	60·9
G .....	7·93	7·55	7·24	5·98	8·06	7·22
B .....	752·88	752·88	752·88	752·88	748·3	748·3
p .....	57·52	46·92	46·5	45·7	47·4	48·1
χ .....	20·4	20·8	21·2	21·6	21·8	22·4
n .....	2 $\frac{1}{3}$	2 $\frac{1}{3}$	2	1 $\frac{3}{4}$	2 $\frac{1}{8}$	2
a .....	3·40	3·3	3·62	3·42	3·72	3·61
τ .....	20·75	20·55	21·35	21·85	22	22·6
T .....	20·86	19·7	21·05	23·15	21·25	21·36
m .....	0·184	0·17	0·164	0·17	0·14	0·133
T <sub>1</sub> .....	32·60	30·67	32·10	32·77	32·53	31·96
m <sub>1</sub> .....	0·014	0·028	0·0166	0·006	—0·005	—0·0025
k .....	0·172	0·159	0·158	0·192	0·130	0·118
α .....	0·0134	0·013	0·0133	0·017	0·0129	0·0128
θ .....	10·83	10·22	10·3	8·75	10·74	10·1
θ <sub>1</sub> .....	10·65	10·02	10·13	8·61	10·59	9·93
t .....	27·08	25·4	26·85	28·35	27·15	26·86
M .....	197·5	200·2	203·25	204·75	195·4	200
Θ .....	170·4	174·8	176·4	176·4	168·25	173·14
c .....	0·4195	0·4267	0·4286	0·4401	0·4251	0·4361

The mean value is 0·4293; the greatest deviation therefrom 0·0108.

The order of experiments was 1, 2, 3, 4, 1a, 2a, 3a, 5, 6, 7, 4a, 5a, 8, 6a. The number 0·4293 does not differ very much from that obtained by Régnault, viz. 0·404 (mean of 0·4147 and 0·3933). Régnault did not carry out experiments between 100° and 20°.

Ethylene, like carbonic acid and nitrous oxide, shows a considerable change in specific heat for changes of temperature. The following true specific heats are calculated:—

For 0° . . . . .	0·3364
100 . . . . .	0·4189
200 . . . . .	0·5015

Multiplying by the specific gravity of ethylene (0·9672) we obtain the relative heats:—

For 100° . . . . .	0·3254
150 . . . . .	0·405
200 . . . . .	0·4851

#### Nitrous Oxide.

The gas was prepared by long-continued heating of ammonium nitrate free from chloride and nitrite; it was purified by passage through caustic potash (to absorb ammonium nitrate carried over and nitric acid) and through sulphuric acid (to absorb free ammonia).

TABLE I.—Experiments between 100° and 25°.

	1.	2.	3.	4.	5.	6.
W .....	59·9	60·29	60·71	60·85	60·53	60·63
G .....	14·69	14·42	16·73	12·18	15·49	14·93
B .....	748·0	748·0	748	753·1	753·9	752·75
p .....	82·4	81·2	67·0	77·5	78·3	76·7
χ .....	19·5	20·0	20·2	23	21·8	23·4
n .....	3½	3½	4½	5	4½	4
a .....	4·2	4·12	3·72	2·44	3·72	3·73
τ .....	19·75	20·2	20·5	23·1	22·1	23·1
T .....	19·7	19·77	20·22	19·2	22·4	22·8
m .....	0·06	0·057	0·075	0·087	0·057	0·056
T <sub>1</sub> .....	27·19	27·1	28·35	25·87	30·24	30·21
m <sub>1</sub> .....	-0·072	-0·045	-0·071	0·01	-0·042	-0·066
k .....	0·059	0·0502	0·070	0·043	0·0608	0·052
α .....	0·0175	0·0137	0·0179	0·0113	0·0131	0·0164
θ .....	7·53	7·38	8·17	6·32	7·8	7·41
θ <sub>1</sub> .....	7·56	7·39	8·18	6·08	7·78	7·44
t .....	23·67	23·62	24·5	22·66	26·5	26·7
M .....	103·5	102	101·8	102	103·8	103·9
Θ .....	79·83	73·38	77·3	79·34	77·3	77·2
c .....	0·2096	0·2138	0·2129	0·2099	0·2144	0·2149

The mean value is 0·2126 ; greatest deviation 0·0030.

Experiments 4, 5, and 6 were carried out after those recorded in Table II.

TABLE II.—Experiments between 200° and 25°.

	1.	2.	3.	4.
W .....	60·38	60·45	60·5	59·92
G .....	9·01	9·18	9·87	8·54
B .....	748·61	748·61	753·3	753·3
p .....	58·34	65·3	58·75	71·4
χ .....	21·6	22·8	21·6	22·4
n .....	2½	2½	3	2½
a .....	3·28	3·44	3·29	3·94
τ .....	22·2	23·4	22	22·35
T .....	19·67	20·85	20	22·55
m .....	0·225	0·21	0·2	0·173
T <sub>1</sub> .....	31·46	33·03	32·85	34·40
m <sub>1</sub> .....	0·072	0·065	0·036	0·022
k .....	0·192	0·180	0·1748	0·166
α .....	0·0130	0·0119	0·0128	0·0125
θ .....	10·82	11·12	12·04	10·94
θ <sub>1</sub> .....	10·46	10·7	11·67	10·74
t .....	25·75	27·24	26·6	28·84
M .....	201	201·45	210·1	212·1
Θ .....	175·24	174·21	183·5	183·26
c .....	0·2248	0·2262	0·2187	0·2267

The mean value is 0·2241 ; greatest deviation 0·0054.

The specific heat found between  $200^{\circ}$  and  $25^{\circ}$  is almost identical with the number 0.22616 obtained by Régnault. From Régnault's experiments carried out for the temperature-interval  $165^{\circ}$  to  $200^{\circ}$ , as well as from my own results, it appears that the specific heat of nitrous oxide increases with increase of temperature.

From the foregoing measurements the following true specific heats are deduced :—

For	0	.	.	.	.	.	0.1083
	100	.	.	.	.	.	0.2212
	200	.	.	.	.	.	0.2442

Multiplying these numbers by the specific gravity of nitrous oxide (1.52) we obtain the relative heats :—

For	0	.	.	.	.	.	0.3014
	100	.	.	.	.	.	0.3362
	200	.	.	.	.	.	0.3712

#### *Ammonia.*

The gas was prepared by gently warming the strongest ammonia of commerce, and dried by passage through a long tube filled with lime.

In order to regulate the velocity of the flow, the gas was passed through a small flask containing mercury and then through an empty flask.

The manometer (*k*) contained concentrated ammonia instead of water during the following experiments. A three-way stopcock served to lead away the excess of gas after leaving the balloon.

TABLE I.—Experiments between  $100^{\circ}$  and  $25^{\circ}$ .

	1.	2.	3.	4.	5.	6.
W .....	60.38	61.45	60.33	59.77	60.9	60.62
G .....	16.12	13.97	13.38	15.12	14.78	16.41
B .....	753.3	752.46	749.62	747.62	749.68	747.4
P .....	60.0	69.04	70.59	73.04	60.32	61.2
$\chi$ .....	19.8	16.3	17.2	17.00	17	25
$n$ .....	4	3	3	$3\frac{1}{2}$	$3\frac{1}{2}$	4
$a$ .....	4.03	4.66	4.46	4.54	4.22	4.10
$\tau$ .....	19.9	16.0	15.9	16.85	16.75	25.15
T .....	17.9	16.38	15.9	16.4	15.8	22.65
$m$ .....	0.08	0.078	0.086	0.101	0.098	0.098
$T_1$ .....	25.8	23.45	23.03	24.39	23.24	30.50
$m_1$ .....	-0.033	-0.05	0.00	-0.0075	0.0025	-0.013
$k$ .....	0.0513	0.0848	0.086	0.095	0.087	0.063
$\alpha$ .....	0.0143	0.0180	0.012	0.0136	0.0130	0.014
$\theta$ .....	7.76	6.79	6.83	7.69	7.24	7.44
$\theta_1$ .....	7.68	6.76	6.7	7.54	7.06	7.28
$t$ .....	22.9	20.0	19.6	20.55	19.6	26.82
M .....	99.8	99.7	99.6	99.6	99.6	102.0
$\Theta$ .....	77.8	79.5	80	79.05	80	75.2
$c$ .....	0.5226	0.5164	0.5246	0.5226	0.5108	0.5173

Table I. (*continued*).

	7.	8.	9.	10.	11.	12.
W .....	59.49	60.23	60.64	60	60.96	66.83
G .....	16.65	16.75	14.35	14.76	15.72	17.7
B .....	747.40	746.6	746.6	747.3	747.3	747.3
<i>p</i> .....	63.24	64.1	71.0	69.0	69.8	71
$\chi$ .....	25.2	24	24.2	23.3	23.7	24.0
<i>n</i> .....	4	4	4	4	4 $\frac{1}{3}$	4 $\frac{2}{3}$
<i>a</i> .....	4.16	4.19	3.59	3.69	3.77	3.73
$\tau$ .....	25.3	23.7	24.2	23.1	23.6	24.1
T .....	21.68	22.25	22.56	21	20.56	21.11
<i>m</i> .....	0.137	0.078	0.09	0.1	0.123	0.126
T <sub>1</sub> .....	29.7	30.05	29.60	28.78	28.95	29.85
<i>m</i> <sub>1</sub> .....	0.002	-0.025	0.0025	0.0066	0.08	0.017
<i>k</i> .....	0.0785	0.059	0.071	0.076	0.085	0.089
$\alpha$ .....	0.0175	0.0131	0.0128	0.0125	0.0125	0.0124
$\theta$ .....	7.49	7.6	6.68	7.35	7.77	8.37
$\theta_1$ .....	7.24	7.5	6.5	7.14	7.49	8.04
<i>t</i> .....	26.0	26.4	26.26	25.1	25.0	25.6
M .....	99.5	101.8	100.96	104	103.5	100.6
$\Theta$ .....	73.5	75.4	74.7	78.9	78.5	75
<i>c</i> .....	0.5084	0.5201	0.5246	0.5245	0.5271	0.5240

The mean value is 0.5202 ; greatest deviation 0.0118.

In experiments 1 to 6 the heating-vessel contained boiling water.

TABLE II.—Experiments between 200° and 25°.

	1.	2.	3.	4.	5.	6.
W .....	60.84	60.71	60.3	60.49	61.04	60.21
G .....	6.2	7.6	9.59	8.2	7.34	9.54
B .....	750.6	750.6	750.6	750.3	750.3	750.3
<i>p</i> .....	45.3	45.3	47.7	37.1	33.0	46.51
$\chi$ .....	23	23	23.6	23.8	23.8	24.1
<i>n</i> .....	2	2 $\frac{1}{2}$	3	3	3	3
<i>a</i> .....	3.1	3.04	3.2	2.73	2.44	3.18
$\tau$ .....	23.06	23.2	23.9	23.8	23.8	24.15
T .....	23.25	22.68	19.78	20.76	21.5	20.75
<i>m</i> .....	0.185	0.204	0.23	0.183	0.20	0.22
T <sub>1</sub> .....	30.9	31.85	31.37	30.5	30.24	31.95
<i>m</i> <sub>1</sub> .....	0.058	0.06	0.075	0.06	0.07	0.054
<i>k</i> .....	0.183	0.195	0.175	0.145	0.165	0.169
$\alpha$ .....	0.0162	0.0157	0.0134	0.0126	0.015	0.015
$\theta$ .....	6.83	8.42	10.55	8.93	7.99	10.45
$\theta_1$ .....	6.59	8.1	10.1	8.57	7.59	10.05
<i>t</i> .....	27.20	27.4	25.73	25.85	26.0	26.52
M .....	203.8	203.9	200.8	201.0	200.0	200.4
$\Theta$ .....	176.6	176.5	175.07	175.15	174.0	173.88
<i>c</i> .....	0.5343	0.5349	0.5291	0.5341	0.5392	0.5340

Table II (*continued*).

	7.	8.	9.	10.	11.
W .....	60.74	60.96	61.01	60.18	60.91
G .....	6.81	7.02	8.15	8.3	6.6
B .....	750.3	745.8	745.8	747.7	747.7
p .....	45.3	51.4	48.9	45.9	44.0
$\chi$ .....	24.4	25.6	26.0	24.8	25.0
$n$ .....	$2\frac{1}{8}$	$2\frac{1}{2}$	3	$3\frac{1}{8}$	$2\frac{1}{2}$
$a$ .....	3.14	2.81	2.72	2.61	2.604
$\tau$ .....	24.4	26.0	26.6	24.5	25
T .....	23.07	23.35	22.55	21.14	21.26
$m$ .....	0.26	0.25	0.30	0.35	0.35
$T_1$ .....	32.01	33.15	32.65	32.0	30.44
$m_1$ .....	0.13	0.135	0.15	0.16	0.17
$k$ .....	0.242	0.22	0.242	0.29	0.277
$\alpha$ .....	0.0145	0.012	0.0148	0.0174	0.0196
$\theta$ .....	7.64	7.95	8.94	9.22	7.13
$\theta_1$ .....	7.22	7.48	8.27	8.40	6.50
$t$ .....	27.6	28.55	27.8	26.6	26.22
M .....	201.0	202.3	199.9	196.9	192.65
$\Theta$ .....	173.4	173.75	172.1	170.3	166.43
$c$ .....	0.5448	0.5493	0.5322	0.5270	0.5323

The mean value is 0.5356 ; greatest deviation 0.0137.

Régnault's number for the specific heat of ammonia between 200° and 20°, viz. 0.50836, is, as he himself supposed, rather too small ; according to my researches it should be 0.5356.

The true specific heats are deduced as follows :—

For	0	. . . . .	0.5009
	100	. . . . .	0.5387
	200	. . . . .	0.5629

Multiplying the numbers by 0.5894 (the specific gravity of ammonia), we obtain the relative heats :—

For	0	. . . . .	0.2952
	100	. . . . .	0.3134
	200	. . . . .	0.3378

The specific heat of ammonia increases therefore with increase of temperature, although not in so marked a manner as in the cases of nitrous oxide, carbonic acid, and ethylene.

### Conclusion.

A comparison of the results obtained by the method described above with those obtained by Régnault, shows that my results are in no respect less exact than his. But inasmuch as the quantity of water in my calorimeter was but the tenth part of that used by Régnault, it was only necessary to employ one tenth of the quantity of gas in order to attain as great an in-

crease of temperature. On account of the saving of material it was possible to give a wider expansion to the research in a comparatively shorter time.

The following Table contains the numbers represented in a somewhat different manner. Columns I., II., and III. contain the true specific heats of the various gases calculated on unit weight. Column IV. gives the difference between the true specific heats at  $0^\circ$  and at  $200^\circ$ , stated in percentages of the specific heat at  $0^\circ$ . The fifth, sixth, and seventh columns give the true specific heats calculated on unit volume, the specific heat of unit volume of air being taken as 0.2389; the eighth column shows the specific gravities of the gases; and the ninth contains the proportion, estimated by Régnault, of the products of the volumes  $V$  and  $V_1$  and the pressures  $P$  and  $P_1$ , when  $P$  is taken as about one atmosphere and  $P_1$  as about two atmospheres. The deviation of the number so obtained from unity (to which it is equal in the case of perfect gases) may be taken as a measure of the deviation from the state of perfect gas.

	Specific heats of equal weights.				Specific heats of equal volumes.				
	I. $0^\circ$ .	II. $100^\circ$ .	III. $200^\circ$ .	IV.	V. $0^\circ$ .	VI. $100^\circ$ .	VII. $200^\circ$ .	VIII. Spec. grav.	IX. $\frac{PV}{P_1V_1}$ .
Air .....	0.2389	...	...	0	0.2389	...	...	1	1.00215
Hydrogen ...	3.410	...	...	0	0.2359	...	...	0.0692	
Carbonic oxide	0.2426	...	...	0	0.2346	...	...	0.967	1.00293
Carbonic acid.	0.1952	0.2169	0.2387	22.28	0.2985	0.3316	0.3650	1.529	1.00722
Ethylene .....	0.3364	0.4189	0.5015	49.08	0.3254	0.4052	0.4851	0.9677	
Nitrous oxide.	0.1983	0.2212	0.2442	23.15	0.3014	0.3362	0.3712	1.5241	1.00651
Ammonia.....	0.5009	0.5317	0.5629	12.38	0.2952	0.3134	0.3318	0.5894	1.01881

As, according to Avogadro's law, equal volumes of different gases contain equal numbers of molecules, it follows that the specific heats of equal volumes of gases are also expressive of the molecular heats of these gases.

The specific heats calculated from the foregoing experiments for constant pressure are made up of two parts:—first, the heat connected with the expansion of the gas for overcoming the external pressure, which is calculated (from the coefficient of expansion and equivalent of heat) to be 0.06902 thermal unit for altering the temperature of 1 gram of air through  $1^\circ$ ; secondly, the heat connected with the internal work of the gas, which might also be calculated from the estimation of the specific heat at constant volumes. The present does not appear

to be the time for making an attempt to estimate the separate parts of the heat of molecular motion—heat of atom, and of atoms (*atom- und Atomenwärme*), according to Naumann—which constitute the specific heat at constant volume, nor for endeavouring to trace simple relationships between these—inasmuch as these relations would appear altogether different for different temperatures, on account of the unequal alterations in specific heats with alteration in temperature, which are not to be entirely explained as small differences in heats of expansion.

The alterations in the specific heats of gases with alterations in temperature cannot be explained by taking into account the deviations from the state of permanent gases. In the case of ammonia, for instance, the alteration with the temperature is much smaller than it is in the case of nitrous oxide and carbonic acid, although the latter are much more nearly permanent gases than the former, as appears from the proportion of the values of  $\frac{PV}{P_1V_1}$ .

The smallness of the difference between the coefficients of expansion of the gases show that the alterations in question cannot be traced to this difference.

Any attempt to explain the alterations in specific heat of these gases from alterations in the specific heat of the elementary bodies composing them, say carbon and nitrogen, does not appear feasible. For, in the first place, the experiments on air show that the specific heat of nitrogen itself does not alter with the temperature; and further, the specific heat of all carbon compounds does not show alterations with the temperature. One must rather accept F. Weber's view\*, that the carbon contained in these compounds is possessed of properties other than those which characterize the carbon of carbonic acid. It is not impossible that variations in the specific heat of carbon may have something to do with the phenomena noticed in the case of carbonic acid.

The alteration in specific heats with the temperature may perhaps be explained by supposing that, when one of the given gases is warmed, a gradual dissociation is taking place which eventually results in a loosening† of the combination of individual atoms, and that this process is attended with the absorption of heat. The fact that the decomposition of nitrous oxide is accompanied with evolution of heat is not opposed to this explanation. Favre regards this evolution of heat as caused by a change of the active oxygen of the oxide into inactive

\* Pogg. *Ann.* 1875, vol. cliv. p. 578; *Phil. Mag.* [IV.] vol. xliv. p. 300.

† Horstman, *Ber. d. deut. Chem. Ges.* ii. 723.

oxygen. At the temperatures employed in the foregoing researches, however, it is not to be supposed that dissociation would proceed to completion ; this change, therefore, would not have taken place.

But further experimental investigations must precede the theoretical discussion of this question. It will be my first endeavour to carry out experiments within greater temperature-limits, and especially to estimate the specific heats of the vapours. I have already carried out a prolonged series of preliminary experiments with this object, making use of the apparatus already described with a few slight modifications ; the narration of these experiments will occupy a further communication.

### XIII. *Model of a small Electrical Machine.*

By GEORGE FULLER, C.E., Belfast\*.

**T**HIS machine is a double-acting electrophorus worked by cranks, with the addition of an arrangement by which a small electric charge given to it is augmented and kept up, so that the dielectric does not require to be recharged.

A is a vulcanite plate supported in a vertical position by two insulated standards, *p, p*.

B, B' are two insulated metallic plates, one on each side of the plate A ; these can be moved together to and from contact with A by means of cranks worked by the handle C.

*m* and *n* are two conductors armed with needles and connected by means of two glass rods, so that they receive the same reciprocal motion from the mechanism by means of the wire, cord, and pulleys, *g, g, g*. The wire *g* is attached to the arm that moves the plate B' ; and the length of the cord and size of pulley are so adjusted that when this is in its extreme position from A, as shown, *m* and *n* are also at one extremity of their motion, as shown ; and when B and B' are in contact with A, *m* and *n* are horizontal. The conductor *n* is not insulated ; and it will be seen by the diagram that *m* and *n* during their motion pass on either side of the plate A.

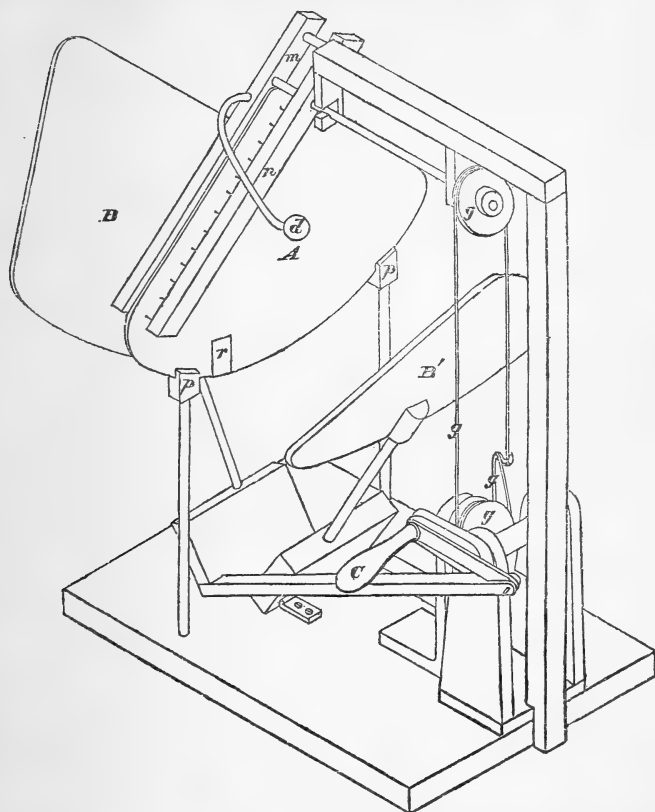
To the conductor *m* is fixed a wire terminating in a brass ball, *d* ; this wire is of such length and form that when B and B' recede from A, and *m* and *n* move downwards from their horizontal position, the ball *d* passes close to B'. Care has to be taken that this juxtaposition does not occur until the vulcanite plate separates the points on *m* and *n*.

The action of the instrument is as follows :—A charge of,

\* Communicated by the Physical Society.



say, negative electricity having been given to the insulated arm *m* when in a vertical position, on motion being given to



the machine this electricity is distributed by means of the points over the face of the vulcanite next to *B* ; at the same time positive electricity is drawn from the earth, thrown upon the other face of that plate by the uninsulated points attached to *n*. When now *B* and *B'* are brought into contact with *A*, the negative electricity on its one face attracts the positive on the conductor *B* and repels the negative, whilst the positive on its opposite face attracts the negative on the conductor *B'* and repels the positive ; then, by means of the piece of tinfoil (*r*) that connects *B* and *B'* when they are in contact with *A*, the two electricities that are repelled neutralize each other ; and when the plates are moved away from the vulcanite, *B* is charged with positive and *B'* with negative electricity. Then, before

reaching its extreme position, the latter communicates its charge to the insulated arm  $m$  by the brass ball  $d$ , and its negative electricity is thus distributed over the surface of A next to B and thus augments its original charge. At the end of its path B' is momentarily connected to earth. It will be evident that on again bringing the plates into contact the charge in B is augmented; also that if a supply of negative electricity is required, the only modification of the above is to give to  $m$  a charge of positive instead of negative electricity.

The above instrument forms an easily constructed and worked electrical machine, and is beside interesting as rendering automatic the classical electrophorus of Volta.

#### XIV. *Views of the Nature of Heat.* By F. MOHR\*.

THE phenomena of heat have been till now almost exclusively explained in text-books by the assumption of a Heat-substance. The discoveries of Melloni have made this view inapplicable to the phenomena of Radiant Heat; they require the assumption of vibrations similar to those of the Undulatory Theory of Light. The Propagation, Transmission, and Polarization of Radiant Heat have been completely explained by these assumptions; and, with such facts to guide us, it is certainly no mere idle speculation to attempt to extend this view to the phenomena of common or stationary heat; rather it is in the highest degree seasonable to point out how this view, which depends upon the well-settled facts of Melloni, explains with overwhelming clearness most of the phenomena of stationary or conducted heat; and it is to be expected that, with this reform in our ideas, there will be a corresponding reform in our terminology.

Heat is thus no longer a particular kind of matter, but an oscillatory motion of the smallest parts of bodies. Radiant Heat is propagated in straight lines; and the molecules vibrate in all directions in a plane perpendicular to the direction of the ray. A polarized ray vibrates in one direction only in this plane; on the contrary [the particles of] an ordinary hot body vibrate in all possible directions of space, and therefore propagate their heat uniformly in all directions. The propagation of heat by contact is thus a communication of motion by impact; and cooling is a relative coming to rest. The number of heat-vibrations per hour must, as in the case of light, be very large, since all bodies become luminous at a certain temperature; but even at this temperature waves of light and

\* Translated by Professor Tait from Liebig's *Annalen*, vol. xxiv. 1837.

of heat are not identical ; their difference has been hitherto just as little cleared up as that between rays of heat from different sources.

The difficult idea of imponderability is at once got rid of, since a body when vibrating cannot be either heavier or lighter than when at rest. The so-called absolute zero [of temperature] is therefore absolute rest—no doubt altogether unknown to us in reality, but presenting no difficulty to the mind.

Heat appears as force (*Kraft*). It overcomes the cohesion of bodies, which is a force ; but *that which overcomes a force must be itself a force*. And the expansion of bodies by heat is a force-phenomenon of the highest kind ; but that which produces motion or manifestation of force must be a force.

The expansion of bodies by heat is thus an extended amplitude of vibration. The bodies themselves do not expand, but only the space they occupy is increased by their larger vibrations. The compression of bodies is due to a change of the amplitude of vibrations by external power. At the absolute zero all bodies must be absolutely incompressible. One can measure the expanding force in certain bodies in terms of weight. Water expands by heating 0.00466 of its volume for 1° C. Experiment shows that water loses about  $\frac{48}{1000000}$  of its volume under one atmosphere of pressure. To expand water thus much by heating there would be necessary  $\frac{0.000048^\circ}{0.004660} = \frac{1^\circ}{97}$  C. The two forces would then be in equi-

librium. Heating by  $\frac{1^\circ}{97}$  C. produces in water a force of pressure of one atmosphere. 1° C. is thus to be considered = 97 atmospheres, and 10° C. = 970 atmospheres.

When a body passes from the solid to the liquid state, heat becomes latent. But one cannot understand how heat can be present in a body without becoming evident to our senses. The usual explanation is that the body is melted ; but this is no explanation, merely the repetition of the fact to be explained. The easier explanation, from our point of view, is as follows : when heat-force is expended in overcoming another force, cohesion, it must itself cease to be perceptible as force ; thus every case in which heat becomes latent is connected with production of motion or with alteration of state of aggregation—that is, with annihilation of material forces. Since matter cannot overcome any force, this explanation was not at the command of the old view. A melted body cannot solidify without giving up to another body the force which destroys its cohesion:—*Evolution of heat in the solidification of melted bodies.*

In the production of gas the same thing takes place. In order to overcome the cohesion of water, some heat must be destroyed and become imperceptible. In the coming back to the liquid state this heat must be again disposable and able to put into vibration, *i. e.* to warm, the thermometer or the hand. A gas vibrates so that its particles always tend to drive one another further off. The mere presence of a gas therefore acts like a material force. In a *solid body* the particles are by the vibration *not* driven out of the sphere of attraction, in a *liquid body partly*, and in a *gas entirely*. When one compresses a gaseous body so as to make its particles move within this sphere of attraction, they attract one another and become liquid :—*Liquefaction of gases by pressure*.

There is an essential difference between gases on the one hand, and the two other states of matter on the other.

In solids and liquids each temperature corresponds to a determinate expansion, or each number of vibrations to a definite magnitude of their amplitude ; on the contrary, in gases the endeavour is at all temperatures to indefinitely increase these amplitudes. This endeavour shows itself as tension (*Spannung*), and appears in gases only. The temperature is the number of vibrations which a body makes in a given time ; this number increases with elevation of temperature. All heat which is not employed for this purpose disappears to our senses, and thus we have the simple distinction between sensible and latent heat : *Sensible heat increases the number of vibrations ; Latent heat increases their amplitude, or overcomes material forces, i. e. destroys states of aggregation.*

When a mass of gas is suddenly compressed, the excursions of the separate particles are diminished ; and thus some of that force becomes disposable which before determined extent of vibration, and was therefore not perceptible by our senses. This force can now be employed only to increase the number of vibrations, since their extent is limited ; that is, the gas becomes warmer. *Apparatus for inflaming tinder by compression of air.* (*Compressionsfeuerzeug.*)

When a gas is suddenly expanded, the separate particles are compelled to make larger vibrations, for which greater force is required. This can only be taken from the force which determines the number of vibrations (temperature), or from surrounding bodies, *i. e.* the gas becomes cooled ; which experiment verifies. Both phenomena are entirely unexplained by the old view.

Wide vibrations have greater force than narrow ; but at high temperatures the vibrations always increase in extent. This increase corresponds, for equal increments of heat, to a definite

fraction of the immediately preceding volume, and not of that at  $0^{\circ}$  C., which temperature has no definite relation to gases. Now, since this fraction must itself become absolutely larger with the already too much increased motion, a greater force must be required to heat a body from  $90^{\circ}$  to  $100^{\circ}$  C. than from  $0^{\circ}$  to  $10^{\circ}$ . This is the already generally observed increase of capacity for heat at high temperatures. All bodies which expand according to this law must show increasing capacity for heat. In the neighbourhood of the melting-point bodies expand rapidly and irregularly; thus much heat becomes latent—most of it, however, during the melting itself. In most cases, in melting, heat is rendered latent in two ways:—by expansion and by melting respectively; in the case of water, which contracts in melting, some heat is by this means given out; or, more correctly, the quantity of that rendered latent is diminished.

If any species of gas is heated more strongly, it strives not only to increase the number of its vibrations, but also to enlarge their amplitudes. If one prevents this expansion, it appears as increased tension. One would require therefore a smaller quantity of heat to warm a gas shut in by firm walls than a gas contained in yielding walls, since, if heat be the cause of the expansion, just as much heat must become latent as there would be cold developed if the gas were allowed to expand as much as before but without the supply of heat. An imprisoned gas, therefore, cannot show the phenomenon of increased capacity for heat. Were we able so to compress solids and liquids that they could not expand by heat, even these would show no increase of capacity for heat; and one could thus save an amount of heat which would, according to the previous calculation, be equal to the compressing force. On this account also the notion of changing, by pressure, liquids into solids is by no means impossible, although we do not as yet know what force would be necessary. The compression of water in a piezometer is in approximation to this.

The increased power of absorption of rough bodies for radiant heat is to be explained by the easily produced vibration of projecting points; while on polished closed planes the very cohesion is opposed to the assumption of vibrations. It would take us too far were we to attempt to reduce all heat-phenomena to this view. Those we have given suffice to show how they are all much more easily and intelligibly deduced from the assumption of vibrations than from that of heat-substance,—and that it is now time to give up, in science and in text-books, the indefinite nomenclature of the earlier view.

[About the time when Colding and Joule took up the experimental investigation of Energy at the point where it had been left

by Rumford and Davy, there were published a great many speculations as to the nature of Heat and its relation to work. Several of these speculations, especially those of Mayer and Séguin, have been discussed, and at least in part reprinted, in the *Philosophical Magazine*. It is right, therefore, that the same journal should recall attention to the above paper, which was recently pointed out to me by Professor Crum Brown, and contains what are in some respects the most remarkable of all these speculations.

Singularly enough, it is not even referred to by Mayer, though his much belauded earliest paper appeared only five years later and in the very same journal. It contains, in a considerably superior form, almost all that is correct in Mayer's paper; and, though it contains many mistakes, it avoids some of the worst of those made by Mayer, especially his false analogy and his *à priori* reasoning.

Polarization of Heat is ascribed to Melloni instead of Forbes; the calculation from the compressibility and expansibility of water is meaningless; and the confusion between the two perfectly distinct meanings of the word *Kraft* is nearly as great as that which some modern British authors are attempting to introduce into their own language by ascribing a second and quite indefensible meaning to the word *Force*.

On the other hand, several of the necessary consequences of the establishment of the Undulatory nature of Radiant Heat are well stated; and the very process (for determining the mechanical equivalent of heat by the two specific heats of air) for which Mayer has received in some quarters such extraordinary praise—though it is in principle, albeit not in practice, utterly erroneous—is here stated much more clearly than it was stated five years later by Mayer.—P. G. TAIT.]

XV. *Some Further Remarks on Helmholtz's Memoir on the Conservation of Force; and on the more Modern Mode of Presenting his Theory.* By ROBERT MOON, M.A., Honorary Fellow of Queen's College, Cambridge\*.

I. **I**N my former paper † I drew attention to Dr. Helmholtz's tacit assumption that, when waves traverse a medium, the term in the equation of *vis viva* depending on the "tensions" vanishes, and to his inference, from a crude and imperfect treatment of a particular case of interference, that the sum of the *vires vivæ* in such cases is always constant.

I showed that, although when a wave is propagated unalterably through a cylindrical tube filled with air, for example, "the sum of the tensions vanishes, and the sum of the *vires vivæ* is necessarily constant, this will not be the case when two such waves, in other respects similar, interfere with each other.

\* Communicated by the Author.

† See *Phil. Mag.* for May 1875.

Suppose, for instance, that we have moving in opposite directions, so as to encounter each other, two condensed waves of the same length, having each a single position of maximum condensation—the condensation at the maximum, and at equal distances from the maximum, being the same in both. Under these circumstances—the particle-velocity throughout the one wave being necessarily in the opposite direction to that throughout the other\*—as the waves meet and gradually overlap, a constant destruction of velocity, and therefore of *vis viva*, will take place in the overlapping portions, till, when the position of complete occultation is attained (that is, when the centre of one wave is coincident with the centre of the other), the *vis viva* of the system of two waves will have entirely disappeared. After passing the position of complete occultation the velocity, and consequently the *vis viva*, will revive, and will go on increasing till the waves are entirely separated, when the *vis viva* of the system of two waves will be exactly what it was at first.

To ascertain the effect of the encounter of the two waves on “the sum of the tensions,” the equation of motion applicable to this case being

$$0 = D \frac{d^2 y}{dt^2} + \frac{dp}{dx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

we shall have for the equation of *vis viva*

$$0 = \frac{D}{2} \int dx \left( \frac{dy}{dt} \right)^2 + \int dt \int dx \frac{dy}{dt} \frac{dp}{dx}; \quad . \quad . \quad . \quad (2)$$

where  $x$  and  $y$  are the ordinates of a particle in the position of rest and at the time  $t$  respectively,  $p$  the pressure at the time  $t$ , and  $D$  the density of equilibrium. The last term of (2) represents “the sum of the tensions;” the integration with respect to  $x$  being supposed to extend over the entire disturbance existing at a given time, that with regard to  $t$  being taken between any epochs we may fix upon.

Before interference we shall have  $\int dx \frac{dy}{dt} \frac{dp}{dx}$  (which call  $\alpha$ ) equal to zero, and the sum of the tensions will vanish. But during the interference the value of  $\alpha$  will vary with  $t$ ; in other words,  $\alpha$  will be a function of  $t$ , and therefore “the

\* It should always be borne in mind that when waves are propagated unalterably the particle-velocity in a wave of condensation will *throughout* be in the direction of propagation; that in a rarefied wave will be *throughout* in a direction contrary to that of propagation,—conclusions which can be readily verified, either with or without the aid of analysis. The velocity at each point will also be always in the same fixed ratio to the condensation, positive or negative, at that point.

sum of the tensions" ( $= \int a dt$  taken between such values of  $t$  as may be fixed upon) will *not* vanish, as is tacitly assumed by Dr. Helmholtz.

Moreover, multiplying (1) by  $\frac{dy}{dt}$ , and integrating with respect to  $x$ , we shall have

$$D \int dx \frac{dy^2}{dt^2} \frac{dy}{dt} + \int dx \frac{dp}{dx} \frac{dy}{dt} = \text{function of } t;$$

and therefore, integrating with respect to  $t$ ,

$$\frac{D}{2} \int dx \left| \frac{dy}{dt} \right|^2 + \int dt \int dx \frac{dp}{dx} \frac{dy}{dt} = \text{function of } t.$$

Hence, if the integration be made between the epochs  $t_1, t_2$  occurring during the period of interference, the right-hand side of the equation will not in general vanish, from which it results that in the case before us, during the period of interference, the sum of the *vires vivæ* and of the tensions will *not* vanish, as asserted by Dr. Helmholtz\*.

We have hitherto considered the results which occur when both the encountering waves are condensed. When both the waves are rarefied, the effect will be precisely similar; but when one of the waves is condensed and the other rarefied, a material variation in the phenomena occurs.

We will suppose, as before, that the waves are of the same length, and have each a single maximum, but that at the middle point, and at equal distances from the middle point, the excess above the mean density in the one is equal in amount to the defect below the mean density in the other. In this case the velocity of each particle of the one wave will necessarily be in the *same* direction as that of each particle of the other.

Under these circumstances, at the moment of complete occultation the condensation at each point will disappear, but the velocity at each point will be doubled; so that the *vis*

\* In my former paper I showed that when, the period of complete occultation being passed, the overlapping is confined to the hinder halves of the waves, the function  $a$  will be negative, whence I too hastily drew the inference that the last term of (1) will be negative. In point of fact in the same way in which it is shown that when the hinder halves only overlap the function  $a$  will be negative, it may equally be shown that when the front halves only overlap the same function will be positive. In fact  $a$ , which will be zero at the beginning, middle, and end of the interference, will be positive during the first half, and negative during the second half; and the sign of the last term of (1), the integration with respect to  $t$  being taken between epochs which occur during the interference, will vary according to the particular epochs we may select.



*viva*, which depends on the squares of the velocities, will be *quadruple* that of either wave taken singly, and the *vis viva* of the system of two waves at the moment of complete occultation will be *double* that of the system of two waves before interference.

If, from the case of oppositely moving waves in a tube filled with air, we turn to that of opposing waves propagated along a stretched cord, we shall arrive at analogous results.

Suppose, for instance, that we have, moving in opposite directions, two waves, each of which till the moment of interference is propagated unalterably, the displacement in each of which is in the same plane, and on the *same* side of the position of rest of the cord. Suppose that the waves are of the same length, that each has a single position of maximum elevation (*viz.* at the middle point), and that at equal distances from the centre the elevation above the position of rest is in each the same.

Thus, before interference, the particle-velocity in the front half (reckoned in the direction of propagation of either wave) will be in a direction *from* the position of rest of the cord, while in the hinder half of each the velocity will be *towards* the position of rest of the cord.

Whence it follows, on the principle of the superposition of small motions, that, when the waves are in a state of complete occultation (that is, when the centre of the one is coincident with the centre of the other), the particle-velocity throughout the whole of the portion of the cord over which the disturbance extends will be zero, at the same time that the distance of each point from its position of rest will be double what it would be at the same distance from the centre of either wave taken singly. In this position of the two waves, therefore, the *vis viva* will entirely disappear.

If, the waves being in other respects as described in the last paragraph, the displacement in the one lies wholly on the one side of the position of rest of the cord, while in the other the displacement lies wholly on the opposite side of the same line, the result of the interference will be different. In this case the portion of the cord occupied by the interfering waves at the period of complete occultation will be in the position of rest, at the same time that the particle-velocity at each point will be double of that at the corresponding point in either wave taken separately. Thus the "sum of the tensions" will be zero, while the sum of the *vires vivæ* in the system of two waves will be double of what it was before interference.

It is difficult to conceive of any case of the point-blank interference of two waves where the results would not be similar to those which have been above pointed out.

Take, for instance, the case of a medium in which the particles, being separated by finite intervals, act upon each other solely by their mutual attractions, and in which the vibration is transversal. If in such a medium two waves, analogous to those already described, advance towards each other, the displacement in the one lying wholly on the one side of the line of direction of the wave, while that in the other lies wholly on the opposite side, it is evident that at the period of complete superposition all particle-displacement must, for the moment at least, have ceased to exist, and all "the tensions" must have ceased to operate. There would, in fact, be an entire annihilation of the disturbance but for the circumstance that each particle, although in the position of equilibrium, will be endowed with a velocity double of that with which it would have been affected if one only of the two waves had reached it. In this case, therefore, the *vis viva* of the system of two waves, instead of being unaltered, will be doubled.

If the disturbance in the encountering waves, instead of lying on opposite sides, is in each case on the same side of the line of direction of the waves, it is obvious that the *vis viva* of the system, which according to Dr. Helmholtz is constant, will at the moment of complete occultation vanish altogether.

II. I now propose to call attention to the singular misnomer involved in the expression "sum of the tensions" adopted by Dr. Helmholtz, which he introduces to our notice in the following terms:—

Taking  $q$  for the velocity of a particle whose mass is  $m$ , and  $\phi$  for "the intensity of the force which acts in the direction of  $r$ ," it is proved that

$$\frac{1}{2} m d(q^2) = -\phi dr,$$

or, when  $Q$  and  $R$  represent corresponding tangential velocities and densities,

$$\frac{1}{2} m Q^2 - \frac{1}{2} m q^2 = -\int_r^R \phi dr.$$

"Let us regard this equation more closely: we find at the left side the difference of the *vires vivæ* possessed by  $m$  at two different distances. To understand the import of the quantity  $\int_r^R \phi dr$ , let us suppose the intensities of  $\phi$  which belong to different points of the connecting line  $ma$  erected as ordinates at these points; then the above quantity would denote the superficial content of the space inclosed between the two ordinates  $r$  and  $R$ . As this surface may be regarded as the sum of the infinite number of ordinates which lie between  $r$

and  $R$ , it therefore represents the sum of the intensities of the forces which act at all distances between  $R$  and  $r$ . Calling the forces which tend to move the point  $m$  . . . the tensions . . . , then the quantity  $\int_r^R \phi dr$  would be the sum of the tensions between the distances  $R$  and  $r$ ." (Taylor's Scientific Memoirs, 1853, p. 122.)

It is somewhat surprising that a mathematician of Dr. Helmholtz's pretensions should have allowed himself to speak of a "surface . . . as the sum of [an] infinite number of ordinates," that is, of an infinite number of geometrical lines! The sum of an infinite number of lines can but be a line, infinite it may be in length, but not a surface.

Dr. Helmholtz's surface is *not* the sum of an infinite number of ordinates, but the sum of the products of an infinite number of ordinates each multiplied by the distance between itself and its next consecutive; in other words, the surface is *the sum of the products of the ordinates by the distances between them*.

Having misconceived the import of the surface which he has thus introduced to our notice, we can hardly be surprised that Dr. Helmholtz should have mistaken the character of the integral of which that surface is a correct representative.

In the differentiations which occur in this part of Dr. Helmholtz's memoir the time is the "primitive variable," a fact which he himself expressly recognizes (*ubi supra*, p. 120).

It follows that the quantities which he has thought proper to write  $d(q^2)$  and  $dr$  would be more fully and precisely expressed by the symbols  $\frac{d(q^2)}{dt} dt$  and  $\frac{dr}{dt} dt$ , and that his equation

$$\frac{1}{2} m d(q^2) = -\phi dr$$

is in effect identical with the following, viz.

$$\frac{1}{2} m \frac{d(q^2)}{dt} dt = -\phi \frac{dr}{dt} dt,$$

and that his equation

$$\frac{1}{2} m Q^2 - \frac{1}{2} m q^2 = -\int_r^R \phi dr$$

is nothing more or less than the following, viz.

$$\frac{1}{2} m Q^2 - \frac{1}{2} m q^2 = -\int_t^T \phi \frac{dr}{dt} dt,$$

where  $T$  and  $t$  are the times corresponding to the distances  $R$  and  $r$ . It follows, therefore, that what Dr. Helmholtz would have us accept as "the sum of the tensions [acting] between

the distances  $R$  and  $r$ ," is in fact the sum of the following products, viz. of the tensions acting (say) at the beginning of each of an indefinite number of equal intervals  $dt$ , into which the interval  $T$  (corresponding to the distances  $R$  and  $r$ ) is divided, each multiplied by the velocity of the particle at the same instant and the short interval  $dt$ , during which the tension and the velocity may be considered uniform.

It is thus evident that the term in the equation of *vis viva* which Dr. Helmholtz offers to our notice as depending solely on the tensions, does in fact depend on the tensions and velocities combined; from which it results that the interpretation of the equation of *vis viva* which forms the basis of Dr. Helmholtz's theory, viz. that the energy of a system consists of two separable constituents, one depending solely on the motion existing in the system, the other depending solely on the forces exerted by the particles on one another, has no foundation in fact.

Who shall say how far the acceptance so widely accorded to Helmholtz's theory is not due to this palpable, though no doubt unconscious, misrepresentation of the true import of the equation of *vis viva*?

The later disciples of Dr. Helmholtz, while altering his nomenclature, have not attempted to eliminate from it its misleading character.

"The kinetic energy of a body is the energy which it has in virtue of being in *motion*, and . . . its value may be found by multiplying the mass . . . and half the square of its velocity." (Maxwell's 'Theory of Heat,' p. 90.)

This is a plain assertion that half the product of the mass and the square of the velocity represents *the whole* of the energy which the body has by virtue of being in motion, which, it is manifest from what has preceded, is not the case.

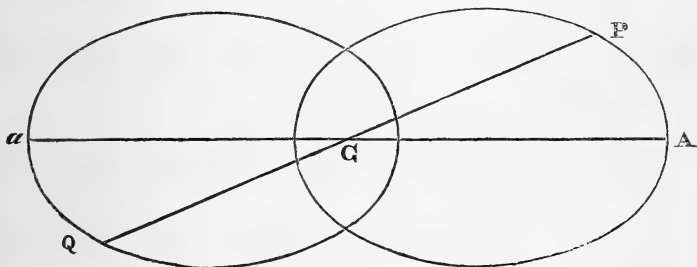
The epithet "potential," as characterizing a kind of energy different from the kinetic, is open to the same objection as Dr. Helmholtz's expression "sum of the tensions;" that is, that it states or necessarily implies what is not the fact, viz. that the quantity which it is intended to designate is independent of the velocities existing in the system.

III. A like confusion of language, and one equally misleading, occurs in a passage to be found in the next page of Prof. Maxwell's Treatise, the consideration of which will serve to exhibit in a striking manner the degree of care and judgment with which the foundations of the theory have been laid, and the amount of discrimination displayed in the criticism which has been bestowed upon it.

After being told that "the energy of a system of bodies

acting on one another with forces depending on their relative positions is due partly to their motion and partly to their relative positions," we are told that "the part which is due to their relative position depends on the work which the various forces would do if the bodies were to yield to the action of those forces." (Ibid. p. 91)

Suppose that we have two equal bodies, P, Q, which, under the influence of their mutual attractions, varying inversely as the distance squared, describe similar and equal ellipses about their common centre of gravity, G, supposed at rest. If, at a given instant, P occupying the position in the figure moves towards A, we shall have Q in the line P G produced moving towards a.



In order to find in this case "the work which the various forces would do if the bodies were to yield to the action of those forces," we must resolve the attractive force on each body in a direction parallel to the direction of its motion, since "the whole effect . . . of work . . . depends on the part of the force which is in the direction of motion" (p. 90).

In the case before us it is evident that throughout portions of each orbit, amounting in the whole to one half, the force on either body parallel to its motion, will act in the direction of its motion; while in the other portions, also amounting collectively to one half, it will act in the direction *opposite* to the direction of motion.

But we are elsewhere informed (p. 89) that "if the force acts on the body in the direction *opposite* to the motion . . . the force, instead of doing work on the body, will be a resistance which the body in its motion overcomes;" and as work is done when resistance is overcome (p. 87), it follows that the work which is done under such circumstances is done *not* by the force but by "the body in its motion"—or, to use somewhat plainer English, by the momentum with which the body is for the time being endowed.

It is evident, therefore, that in the case we have just been

considering it is not true, as asserted by Prof. Maxwell, that the energy of the system consists of two parts—one depending on the motion of the body represented by the kinetic energy, the other depending on the work which the forces would do if the bodies were to yield to their action, since during this portion of the motion the body cannot yield to the action of the forces, that action being controlled and overcome by the momentum for the time being inherent in each body.

If the second of the two components of which the total energy is assumed to consist is to be expressed in the language of the 'Theory of Work,' it is evident that during one half of the motion in the last case, Prof. Maxwell's account of the composition of energy must be modified into some such terms as the following.

The energy of the system consists of two parts—(1) the kinetic energy which bodies have by virtue of being in motion, (2) an energy depending on the amount of work which the momentum in the system would do if the bodies were to yield to its action,—a classification which, however exactly it meets the case to which it is intended to apply, is entirely deficient in that antithetical character which the epithets "kinetic" and "potential" would assign to the two functions which are assumed to represent the total energy, and which has gone so far to seduce the judgment of those who have given in their adhesion to the theory.

IV. A more obvious and decisive contradiction of the theory will be afforded by writing down the equation for the *vis viva* in the foregoing case of motion.

Taking unity for the mass of each body, the equations applicable to the motions of P will be of the form

$$\frac{d^2x}{dt^2} = -\frac{\mu x}{r^3},$$

$$\frac{d^2y}{dt^2} = -\frac{\mu y}{r^3},$$

the coordinates being measured from G parallel and perpendicular to Aa. Hence

$$\begin{aligned} \frac{1}{2} \left( \left| \frac{dx}{dt} \right|^2 + \left| \frac{dy}{dt} \right|^2 \right) &= - \int_t \frac{\mu}{r^3} \left( x \frac{dx}{dt} + y \frac{dy}{dt} \right) \\ &= - \int_t \frac{\mu}{r^2} \frac{dr}{dt}, \end{aligned}$$

or

$$\frac{v^2}{2} = \frac{\mu}{r} + C,$$

where  $r$  is necessarily positive. The expression for the *vis viva* of  $Q$  will be precisely the same; so that the equation of *vis viva* for the system of two bodies will be

$$v^2 - \frac{2\mu}{r} = 2C.$$

It thus appears that instead of the term depending on the tensions being always positive, as the theory assumes, it will, in the case just considered, be always negative; so that if we take the *vis viva* to represent the kinetic, and the term depending on the tensions the potential energy, it follows that it is the *difference*, not the *sum* of the kinetic and potential energies which in this case of motion will be constant.

I apprehend that the same will hold in all cases where particles separated by finite intervals attract each other by forces varying as some inverse power of the distance greater than the first. A simple instance which any one may without difficulty try for himself is, where two equal bodies are moving in the same straight line towards their common centre of gravity.

The theory of the Conservation of Force or Energy professes to be founded on the principle of *vis viva*, of which, in the cases to which the latter applies, the principle of the conservation of energy purports to be merely a translation into other terms.

If the foregoing investigation be correct, it is evident that the principle of the conservation of energy (in the form in which it has been propounded) must involve a mistranslation of the principle of *vis viva*, and that, instead of deriving support from, it is emphatically contradicted by the latter.

6 New Square, Lincoln's Inn,  
March 30, 1876.

## XVI. On the Optical Properties of Starch.

By WALTER BAILY, Esq\*.

WHEN a grain of starch is seen under the microscope, it generally exhibits a spot surrounded by a series of faint lines forming closed curves. As the spot never occurs on the apparent circumference of the grain, whatever position the grain may take, the spot is obviously internal; and as the lines always surround it in the same manner, it follows that they are not fixed markings but the foreshortened parts of a series of transparent envelopes. In fact the grain consists of a nucleus surrounded by coats.

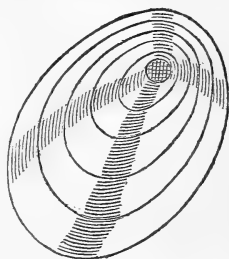
\* Communicated by the Author.

Under the polariscope, with the Nicols at right angles, the grain appears to be marked with a black cross, whose intersection always coincides with the nucleus, whose arms reach the circumference, and whose form depends on the kind of starch, as well as on the position of the grain; the spaces between the arms are illuminated with less than full light, the brightness increasing with the distance from the cross. If the Nicols are made parallel the cross becomes white, and the spaces between the arms become dark but not black, the darkness increasing with the distance from the cross. Hence it is evident that the light which emerges from the starch at points on the cross is plane-polarized in the plane of polarization of the lower Nicol, and that the light from points in the spaces is elliptically polarized, the polarization being most nearly circular at points most distant from the cross, where the turning of the upper Nicol was found to occasion the least change in the illumination. In round grains, such as young grains of potato-starch, or in oval grains seen endways, the cross is rectilinear and rectangular, the arms being parallel and perpendicular to the plane of polarization. The appearance resembles closely that given under the polariscope by the circular disks formed in crystallized hippuric acid and salicene. In hippuric acid the pattern is in black and white; but in salicene, though the cross is black or white, the remainder is brilliantly coloured. In starch the pattern is in black and white generally, but the largest grains show a trace of colour.

Now the optical properties of these disks can be completely explained by supposing that the substance is doubly refracting, with two axes of elasticity at each point in the plane of the disk, one of them being directed towards the centre of the disk. The arrangement of the axes of elasticity in the disk is therefore given by a family of lines radiating from the centre, and a family of circles described about it. Plane-polarized light passing through such a disk will emerge without change along the diameters which lie parallel and perpendicular to the plane of polarization; for the vibration will coincide with one of the axes of elasticity at every point on these diameters: but at other points the light will be resolved into a vibration parallel to each axis; and as these two vibrations will be unequally retarded, the light will become elliptically polarized; and the change will be greatest in middle of the quadrants, where the axes are equally inclined to the plane of polarization. The absence of colour in hippuric acid is due to the difference of retardation being proportional to the wave-length, whereby light of all kinds emerges similarly polarized; but in salicene the proportion between the retardation and the wave-length must be different for different kinds of light.



In an oval grain of starch seen sideways the nucleus is situated generally towards one end, and the arms of the cross are curved, except when the length of the oval is parallel or perpendicular to the plane of polarization, in which case two arms form a straight line along the length of the oval, and the other arms are curved. The same effect would be produced by a crystalline disk constructed as follows:—Round a point as nucleus draw a family of ovals, each having the point towards one end, and its greatest length in a certain direction, and let the axes of elasticity in the disk be at each point parallel and perpendicular to the curve through that point. The form of the cross for any position of the lower Nicol will be given by lines drawn through all the points at which the curves are parallel or perpendicular to the plane of polarization.



Now in a round grain, or an oval grain seen endways, the foreshortened parts of the coats give a series of circles, and the optical effects are those of the circular disks considered above; and if the oval grain is seen sideways the foreshortened parts of the coats give a series of ovals, and the optical effects are those of the oval disk. Let us assume that the coats are doubly refracting, and that one axis of elasticity at each point is normal to the coat, and differs in value from the other two which lie in the tangent plane to the coat. These two will of course be of equal value in spherical grains; but in other cases they may differ somewhat from one another, but less than each differs from the normal axis. In each coat, at points where the coat is not foreshortened, the normal axis will be more or less in the direction of the ray, and the effect of the coat on the light will be little or none, since the tangential axes are nearly, if not quite, equal; but where the coat is most foreshortened the normal axis will be at right angles to the ray, and the effect of the coat on the light will be greatest. Hence the effect of the whole grain on the light will be nearly the same as that of a section taking in the nucleus and those parts of the coats which are most foreshortened. But in such a section the optical structure would be exactly that of the disks we have been considering, and which we have seen to produce the same effect on light that a grain of starch produces; and therefore we may conclude that a grain of starch really consists of a series of coats round a nucleus, each coat being doubly refracting, and having one axis of elasticity

normal and two tangential, and that the normal axis differs more from the others than they differ from one another.

Some kinds of starch have grains of more irregular shape than those treated of; but their optical effects may be explained in the same manner.

This conclusion seems to agree with that arrived at by Nügli and Schwendener: see Sach's 'Textbook of Botany,' p. 588.

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## XVII. *The Pitted Surface of Meteorites.*

By N. STORY MASKELYNE, F.R.S. &c.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

IN the *Comptes Rendus* for April 24th of this year M. Daubrée, the eminent director of the École des Mines, has offered an explanation of the hollows which characterize the crusted surface of meteorites. This explanation he partly draws from the very singular parallelism between this alveolar surface of meteorites and that presented by the fragments of unburnt gunpowder that fall at some distance from the muzzle of a large piece of ordnance.

I had myself the pleasure of introducing this subject to the notice of my friend M. Daubrée when he was staying with me in London in the summer of last year. I drew his attention to the remarkable similarity which these incompletely burned fragments of gunpowder bear in the pitted character of their surfaces to meteorites, whose black crust presents hollows and irregularities curiously identical in feature with those of the powder fragments.

I spoke, indeed, with some hesitation as to the causes of this similarity in the two cases, as the conditions were by no means identical. Had M. Daubrée's explanation been in all respects in accord with what seemed and seems to me the most satisfactory way of accounting for phenomena so similar, it would be unnecessary for me to make any further observation on the matter.

I should merely have been anticipated in the publication of one of the points I have discussed in a little treatise on meteorites long nearly ready for printing.

Since, however, M. Daubrée does not give the same explanation of the hollows pitting the surface of a meteorite that still appears to me to be, on the whole, the best way of accounting for them, I venture to put on record, earlier than I otherwise had intended doing, what I conceive is the pro-

bable cause of the variolar surface in the two cases, and in what respects I find myself not in accord with the conclusion of the Director of the École des Mines.

And I should here say that I have had, on more occasions than one during the last two or three years, to thank my friends Professor Abel and Major Noble for specimens of these fragments of gunpowder as projected from the 35-ton and 80-ton guns at Woolwich, and that in the explanation I offer of this part of the questions at issue my opinion has been brought into its present shape by the free communication of theirs, of which I trust it will be a fair representation.

The fragments are generally about the size of a small nut, and very irregular in form; but where the original grain of the gunpowder had a definite geometrical shape, this shape is distinctly recognizable in the diminished fragment which represents it.

The surface of the fragment is usually covered with a sort of irregular reticulation composed of hollows which, though sometimes isolated and nearly hemispherical, more often are seen to become confluent while remaining comparatively shallow. Sometimes a surface is met with that is smooth and unindented. Any one familiar with the aspect of a stone meteorite (an aerolite) will recognize at once the similarity of this description to that of the black incrustated and indented surface of one of these curious bodies. Those who are unfamiliar with this very characteristic feature of a meteorite can see it abundantly illustrated in the collection at the British Museum. Such, I may add, is the resemblance of the gunpowder fragment to a small meteorite, that three years ago, when I placed one of these powder-grains in the hands of Mr. T. Davies, the experienced assistant in the Mineral Department at the British Museum, he exclaimed, "What a beautiful little specimen of the Cold Bokkeveldt meteorite! how remarkably well it is indented!"

One would suppose that such a similarity in the two kinds of object must both be due to a similar cause, and the more so as in each case we have an accompaniment of detonation with enormous velocity and sudden differences of temperature and pressure; and yet this parallelism of cause may nevertheless not be quite so complete as is that of the effects in the two cases.

Among the many splendid services which Professor Abel has contributed, not only to the War Department at Woolwich, but at the same time to science, rank his studies, extended over many years, of the various conditions for and results of the combustion of explosive agents, including gunpowder.

He showed that by simply removing the gases rapidly enough, as in an exhausted vessel, you might arrest entirely the combustion of gunpowder—and, further, that another element in the rapidity with which the combustion penetrates a solid mass of that explosive is the density of the mass. The huge cubes of nearly two inches, no less than the other varieties of “pebble” and “prismatic” forms of coarse-grain powder employed in our large guns are, by their very manufacture, lacking in mechanical homogeneity.

Within the gun, where the pressure is some twenty-two or twenty-three tons to the square inch, the process of combustion goes on rapidly, but can be regulated by adjusting the density; and size of the gas-producing grains of gunpowder. It furthermore goes on with greater intensity at some than at other parts of the surface, in consequence of the variations in density; and at these points centres of combustion become established—the relative depths in different parts of the surface to which the combustion would penetrate in any brief period depending on the relative densities of the ingredient granules or fragments of “cake,” which give to the gunpowder mass the character of a sort of breccia. At the discharge of the gun the released gases escape, together with a few fragments, which are the cores of some of the cubes of gunpowder which have not had time to be entirely consumed; and at the surface of these fragments the combustion will proceed at different centres of combustion, the depth of which depends on the density of the powder and the pressure exerted on it by the surrounding gases.

At the same time the sudden removal of the vast pressure in operation within the gun, and an initial velocity of 1400 feet in a second, sufficiently explain, on Professor Abel’s principle, the extinction of the combustion of these projected fragments. Their pitted surfaces are also explained as the result of mechanical heterogeneity in the component particles of the powder.

With these points M. Daubrée appears to be in accord, though he does not seem to have been aware of Professor Abel’s researches, as he does not refer to them.

Let us now turn to the case of a meteorite. It enters our atmosphere with at least a planetary velocity, which is at once arrested by the resistance the body experiences in traversing even the highest and rarest region of the aerial envelope of our globe.

The resulting heat at once fuses the surface of the stone or iron; and though the fused and oxidized material is thrown off (aided in most cases by a rotatory movement in the mass) as

rapidly as it is formed, the heat must quickly begin to penetrate towards the interior of the body, even where its ingredients offer so inferior a conducting-material as spherules of magnesian or ferro-magnesian silicates united in different degrees of aggregation, and, indeed, often forming a very incompact structure. The explanation I have heretofore offered of the "pitting" of the surface of a meteorite was, that the want of homogeneity of the mass permitted the heat to penetrate more rapidly from its exterior in some places than in others, so that the sudden expansion due to the almost instantaneous accession of enormous temperature on the surface of the stone tore out small pieces of it and flung them off from the swiftly moving mass, the interior of which must be presumed to be the more brittle from the meteoric body entering our atmosphere with the temperature of the "cold of space" (see *Phil. Mag.* January 1863). Even were one large outer shell from this cause to break away from an inner core, we should expect the surfaces to present projections and depressions corresponding in part to the denser or less dense, the better or the less well conducting parts of the mass, and partly also of course to any directions of weaker cohesion in the stone arising from other causes.

The greater fusibility of some than of other of the mineral ingredients of the meteorite (for instance, of the more ferrous as compared with the less ferrous and more magnesiferous silicates) may in certain cases explain some of the peculiar pitlike indentations, as, for instance, those in which a rather deep irregularly shaped hole is met with, perhaps half an inch in depth; and in the fusion and reincrustation of a freshly broken surface, after explosion and fracture, this cause possibly helps in increasing the variolar irregularities of the surface. Another cause of these irregularities suggests itself in the differences of combustibility of the ingredients of a meteorite; for, in a strict sense of the word, in most meteorites the bulk of the ingredients—all in fact except the purely magnesian or calcio-magnesian iron-free silicates—are combustible, *i. e.* can undergo further oxidation; for this is true, be it remembered, not merely of the iron and the troilite ( $\text{FeS}$ ), but also of the magnesio-ferrous silicates bronzite and olivine. But I cannot believe that this is an active agent in the pitting of the surface, however it may contribute to the brilliancy of the fiery meteor. For it is no uncommon thing to find enclosed in the black oxidized glaze of a meteorite little *pepites* of nickel-iron unoxidized; and in the Busti meteorite the yellowish glaze covered alike the augite and the Oldhamite ( $\text{CaS}$ ), without offering any indication of the latter having been burned away

and of hollows being formed at the spots where the crust enclosed or was underlain by spherules of the sulphide.

In general I see no reason to believe that the oxidation penetrates below the thin pellicle of oxidized crust or enamel that perpetually forms over and protects the interior of the stone or iron mass, and so often even encloses unchanged particles of its most oxidizable ingredients. That the character of the pitting which the surface of a meteorite undergoes does not depend so much on the combustibility of its ingredients as on their mechanical aggregation, seems further to follow from the fact that the carbonaceous meteorites are not more pitted (generally, I think, less so) than those which are free from carbon, while, on the other hand, the more compact meteorites, but, I should add, also those containing least of felspathic ingredients (the incrustation of the more felspathic varieties being usually a once very fluid glaze), are those which offer the most pitted surfaces. The meteorites of Iowa, Gopalpur, Pultusk, and the marbled or brecciated stones of Maza Koorna, Aldsworth, and Parnallee may be instanced in confirmation of this ; while the softer and less compact aggregates of meteoritic matter, of which the Nellore and Busti meteorites are examples, present but little pitting on their surfaces.

I believe, then, that it is to the difference in the mechanical facility with which the sudden heat penetrates the mass at different points on its surface, partly as a consequence of greater conductivity, partly of greater fusibility, that the melting-out and immediate dissipation in the air of the material at those parts is mainly due, and that it is in no degree due to the greater combustibility of the mass at those points.

M. Daubrée, on the other hand, does not believe that the "pitting" is due to the sort of splintering effect of enormous heat suddenly applied, because he has failed in producing an analogous surface by plunging various kinds of rock into a furnace at the Conservatoire des Arts et Métiers ; in which, however, the contrasts of temperature and mechanical means of momentarily removing the fluid incrustation are absent from the experiment, even if the characters of the rocks were strictly comparable. M. Daubrée, however, offers another explanation of the phenomenon ; and this at least is one which it would be difficult to refute by the repetition of the conditions in an ordinary experiment. His view is, that the hollows formed on the surface of the meteorite are produced by a sort of boring action effected by the air compressed and fiercely agitated into gyratory motion by the rapidly moving stone. "En tourbillonnant ainsi sous de telles pressions, l'air tend à tarauder," he says, and considers this mechanical action to be assisted by the che-

mical combustion of the iron and troilite contained in the meteorite. The last point has been already considered ; on the other I have only to observe that, were the hollowing-out of a meteorite due to such little whirlwinds on its surface, we should find traces of the rotatory action in a whorl-like distribution or marking of the crust. We do not find this, whereas we do find very often distinctly marked traces of the orientation of the meteorite at a given period in its course, indicated by the backward flow of the then molten crust, which, like some old lava stream *in petto*, still presents lines following the course of that flow on the surface, and a denser incrustation in the rear than on the front, from which the backward-streaming lines take their course.

The subject here discussed, however, is bound up with questions as to the original form in which meteorites enter our atmosphere, questions that have recently been discussed from two very opposite points of view by F. Mohr in Liebig's *Annalen* (vol. clxxix. pp. 257-282), and by Professor Tschermak, in the June Supplementary Number of the Philosophical Magazine for this year. I will take a nearly opportunity of asking you to record my views on both of them.

I am, yours &c.,

N. STORY MASKELYNE.

112 Gloucester Terrace, W.,  
July 17, 1876.

### XVIII. On the Elasticity of Brass.

By Lieut.-Col. A. R. CLARKE, C.B., R.E., F.R.S., &c.\*

IN the course of a recent measurement of the distance between the knife-edges of Kater's Pendulum, it became necessary to consider the question of the change of length which takes place when the position of the pendulum is changed from vertical to horizontal, and also the alteration in length caused by interchanging the knife-edges in swinging. The values of the modulus of elasticity which are found in the ordinary books of reference are given, so far as I have seen, without any statement of the authority on which they rest ; and it does not appear that any precise experiments have been made in this country on the elasticity of brass, though those of iron and steel are well determined. Some circumstances having led me to doubt the accuracy of the value taken from the Table, a simple experiment on the flexure of a brass scale was made, which gave a very different value.

In order to get some precise results, I obtained four speci-

\* Communicated by the Author.

mens of brass, of which the dimensions and weights are shown in the following Table:—

Rod.	Length.	Breadth of opposite faces I. and III.	Breadth of opposite faces II. and IV.	Weight.
No. 1.	in. 39·85	in. 0·2370	in. 0·2400	lb. 0·72161
„ 2.	39·85	0·2392	0·2385	0·73018
„ 3.	39·85	0·2875	0·2127	0·76143
„ 4.	39·85	Circumference = 1·0610		1·09018

Nos. 1 and 2 were cut from the same sheet of brass, but the latter was well hammered; No. 3 is cut from a different sheet. These three specimens were very carefully filed, and their dimensions are very uniform throughout. The quantities in the Table result each from ten measures at equal intervals; and the probable error of each quantity may be  $\pm 0\cdot0003$  in. The round rod No. 4 is very fairly circular in section; it is probably drawn, not rolled. The probable error of its circumference may be taken at  $\pm 0\cdot001$  in.

In order to ascertain their elasticity, the rods were provided with three supports, one at each extremity, and a third at the centre capable of movement by a micrometer-screw in a vertical direction. Thus, when the rod is straight, or when the centre support is in line with the others, the bar rests at the left for one fortieth of an inch of its length, and over its entire breadth, upon the brass rectangular support; the central movable support is a knife-edge; and at the right end the contact is on a surface of one fortieth of an inch square, being in the mid breadth of the rod. Now if  $w$ ,  $h$ ,  $k$ ,  $l$  are the weight, breadth, depth, and length, and  $E$  the modulus of elasticity of the rod,  $\frac{1}{2}ln$  the breadth of each support, so that  $l(1-n)$  is the distance between the edges of the supports, then

$$e = \frac{wl^3}{4Ehk^3} \left(1 - \frac{5}{2}n\right),$$

where  $e$  is the space (measured by the micrometer-screw) moved over by the centre of the rod between its position of resting wholly on the supports at the ends, and the position of being carried entirely by the centre support and having just contact only with the extreme supports. The three supports were fixed in a window-sill, the greatest rigidity being requisite in order to obtain trustworthy results. The experiments were all made at nearly the same temperature ( $65^\circ$  Fahrenheit); and great care was taken to avoid change of temperature during the measures. The faces of the rods were numbered I.,



II., III., IV.; and the flexure was measured with each face up; so that each bar gives four values of  $E$ : the round rod was also observed in four positions. In the case of this rod,

$$E = \frac{wl^3}{12\pi ek^4} \left(1 - \frac{5}{2}n\right),$$

where  $k$  is its radius.

To combine the four values of  $E$  in each case, we should proceed as follows:—if  $dw, dl, \dots$  be the corrections necessary to the observed values of these quantities, we must express the consequent corrections to the computed values of  $E$ . Let the first two (that is, the values of  $E$  resulting from the opposite faces I. and III.) be multiplied each by a factor  $\frac{1}{4}(1 + \theta)$ , and the other two by  $\frac{1}{4}(1 - \theta)$ ; then it is easy to express the probable error of the resulting value of  $E$  in terms of the probable errors of the observations; and the value of  $\theta$  to be used is that which renders this expression a minimum. In the present case, however, the first two rods are very nearly square in section; and the third rod giving identical values (or nearly) for  $E$ , the multipliers are immaterial. We therefore take the mean of the four computed values with a probable error

$$\pm E \left\{ \frac{\partial w^2}{w^2} + 9 \frac{\partial l^2}{l^2} + 4 \left( \frac{1}{h^2} + \frac{1}{k^2} \right) \partial h^2 + \frac{25}{4} \partial n^2 + \frac{1}{f^2} \partial e^2 \right\}^{\frac{1}{2}}$$

for the square rods, and

$$\pm E \left\{ \frac{\partial w^2}{w^2} + 9 \frac{\partial l^2}{l^2} + 16 \frac{\partial k^2}{k^2} + \frac{25}{4} \partial n^2 + \frac{1}{f^2} \partial e^2 \right\}^{\frac{1}{2}}$$

for the round rod, where

$$\frac{1}{f^2} = \frac{1}{16} \left( \frac{1}{e_1^2} + \frac{1}{e_2^2} + \frac{1}{e_3^2} + \frac{1}{e_4^2} \right),$$

the symbol  $\partial$  meaning probable error;  $\partial h$  is taken to be the probable error of either  $h$  or  $k$ ; and the four  $e$ 's are assumed to be determined with the same accuracy.

This last supposition is not, however, quite exact. In the following Table the values of  $e$  are given for each face of each rod, the probable error being that resulting from the agreement of the eight determinations made with each face up.

Rod.	Face I. up. Face III. up.	Face II. up. Face IV. up.
	in.	in.
No. 1. {	0·20670 +3	0·21328 +2
	0·20716 +4	0·21342 +2
„ 2. {	0·20943 +1	0·20831 +2
	0·20885 +4	0·20803 +2
„ 3. {	0·26910 +3	0·14719 +1
	0·26910 +1	0·14715 +2
„ 4. {	0·14875 +2	0·14882 +2
	0·14853 +5	0·14912 +1

The probable errors here refer to the last figure of the preceding decimal. An examination of this Table shows that there are, notwithstanding the extreme care with which the observations were made, some unknown constant errors in existence outweighing the apparent probable errors of the different measurements, and therefore interfering with the application of the theory given above, at least for rods Nos. 1 and 2. I hope by some future observations to discover the source of the constant errors in question. The following Table gives the values of E for each rod expressed in millions of pounds.

Rod.	Face I.	Face III.	Face II.	Face IV.	Mean.
No. 1.	16·806	16·769	16·737	16·726	16·759
„ 2.	16·945	16·992	16·937	16·960	16·958
„ 3.	16·181	16·181	16·192	16·196	16·188
„ 4.	15·083	15·105	15·076	15·045	15·077

While making these experiments I met with a very elaborate work published at St. Petersburg, entitled *Recherches Expérimentales sur l'élasticité des Métaux faites à l'observatoire physique central de Russie*, par A. T. Kupffer. Of brass, nine specimens were experimented on—three of cast brass, three of plate brass, and three of hammered. The experiments were made :—first, by causing the rods to bend under given weights, and measuring with vertical circles the inclinations of the ends of the rods so bent ; secondly, by causing the rods to vibrate transversely when fixed by one end in a vertical position, the other end being weighted, and counting the vibrations. The results obtained by the two methods agreed very satisfactorily. The following Table gives the moduli of elasticity expressed in millions of pounds for the nine specimens.

Cast.	Rolled.	Hammered.
12·262	15·338	16·012
11·537	16·244	16·523
14·540	15·884	15·753

These quantities result from a very large number of observations.

XIX. *On the Extra Current.* By OLIVER HEAVISIDE\*.

LET a wire possessing uniform electrical properties throughout be of length  $l$ , resistance  $kl$ , electric capacity  $cl$ , and let its coefficient of self-induction be  $sl$ . Further, let P and Q denote the two ends of the wire, and  $x$  the distance of any point from the end P. Let  $v$  be the electric potential at the point  $x$  at the time  $t$ , and  $Q$  the quantity of electricity that has passed that point from the time  $t=0$ , so that  $\frac{dQ}{dt}$  is the current.

The differential equation of the potential of the wire may be found from the following two equations:—

$$-\frac{dQ}{dx} = cv, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$-\frac{dv}{dx} = k \frac{dQ}{dt} + s \frac{d^2Q}{dt^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The first expresses the fact that the quantity of electricity existing on the surface of the wire between sections at  $x$  and  $x+\delta x$  at any moment is the product of the potential and the capacity of the portion of wire considered. The second expresses that the electromotive force at the point  $x$  at any moment is the sum of the electromotive force producing the current  $\frac{dQ}{dt}$  and the rate of increase of the momentum of that current. By eliminating  $Q$  we obtain

$$\frac{d^2v}{dx^2} = ck \frac{dv}{dt} + cs \frac{d^2v}{dt^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If  $s=0$ , the above equation becomes

$$\frac{d^2v}{dx^2} = ck \frac{dv}{dt},$$

the differential equation of the linear flow of heat, or of electricity in a submarine cable, the practical solution of which for a wire of finite length can only be accomplished with the assistance of Fourier's theorem. And if  $k=0$ , we have

$$\frac{d^2v}{dx^2} = cs \frac{d^2v}{dt^2};$$

which is of the same form as the equation of motion of a vibrating wire, the solution also requiring the use of Fourier's theorem. It is therefore probable that the same method must

\* Communicated by the Author,

be adopted to solve the equation under consideration, viz.

$$\frac{d^2v}{dx^2} = ck \frac{dv}{dt} + cs \frac{d^2v}{dt^2}. \quad (3)$$

Let the potential of the wire at any moment be

$$v = V \frac{\sin i\pi x}{\cos l} \cdot f(t), \quad (4)$$

where  $f(t)$  is a function of  $t$  only, and  $V$  is constant. From (4) by differentiation,

$$\frac{d^2v}{dx^2} = -\frac{i^2\pi^2}{l^2}v;$$

therefore by (3),

$$cs \frac{d^2v}{dt^2} + ck \frac{dv}{dt} + \frac{i^2\pi^2}{l^2}v = 0,$$

the solution of which is

$$v = \epsilon^{-\frac{t}{2\alpha}} (A \epsilon^{\frac{t}{2\alpha}} \sqrt{1 - 4i^2\pi^2 \frac{\alpha}{\beta}} + B \epsilon^{-\frac{t}{2\alpha}} \sqrt{1 - 4i^2\pi^2 \frac{\alpha}{\beta}})$$

if  $4i^2\pi^2 \frac{\alpha}{\beta} < 1$ , and

$$v = \epsilon^{-\frac{t}{2\alpha}} (A' \cos + B' \sin) \frac{t}{2\alpha} \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}$$

if  $4i^2\pi^2 \frac{\alpha}{\beta} > 1$ . Here  $A$ ,  $B$ ,  $A'$  and  $B'$  are constants, and  $\alpha = \frac{s}{k}$  and  $\beta = ck l^2$ , both time-constants. Therefore if the potential when  $t=0$  is

$$v = V \frac{\sin i\pi x}{\cos l},$$

the potential of the wire at the time  $t$  is

$$v = V \frac{\sin i\pi x}{\cos l} \cdot \epsilon^{-\frac{t}{2\alpha}} \cdot \{A \cdot \epsilon^{\frac{t}{2\alpha}} \sqrt{1 - 4i^2\pi^2 \frac{\alpha}{\beta}} + (1-A) \epsilon^{-\frac{t}{2\alpha}} \sqrt{1 - 4i^2\pi^2 \frac{\alpha}{\beta}}\}, \quad (5)$$

or

$$v = V \frac{\sin i\pi x}{\cos l} \cdot \epsilon^{-\frac{t}{2\alpha}} (\cos + B' \sin) \frac{t}{2\alpha} \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}, \quad (6)$$

according as  $4i^2\pi^2 \frac{\alpha}{\beta} < \text{or} > 1$ . The remaining constants  $A$  and  $B'$  must be determined from the value of the current at some fixed time. By solving equation (2), where  $-\frac{dv}{dx}$  is to be found from (5) and (6), we shall find

$$\frac{dQ}{dt} = C\epsilon^{-\frac{t}{\alpha}} + \frac{Vi\pi}{kl} \cos \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \left\{ \frac{2A\epsilon^{\frac{t}{2\alpha}} \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}}{1 + \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}} + \frac{2(1-A)\epsilon^{-\frac{t}{2\alpha}} \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}}{1 - \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}} \right\},$$

or

$$\frac{dQ}{dt} = C\epsilon^{-\frac{t}{\alpha}} + \frac{Vi\pi}{kl} \cos \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \cdot \frac{\beta}{2\alpha i^2 \pi^2} \cdot$$

$$\left\{ \left(1 - B' \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}\right) \cos + \left(B' + \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}\right) \sin \right\} \cdot \frac{t}{2\alpha} \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1},$$

where C is a constant current. Let the initial current be C, then

$$A = \frac{1 + \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}}{2 \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}}, \quad B' = \frac{1}{\sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}};$$

therefore the expressions for the potential and current become

$$v = V \frac{\sin}{\cos} \frac{i\pi x}{l} \cdot \frac{\epsilon^{-\frac{t}{2\alpha}}}{2m} \{ (1+m)\epsilon^{\frac{tm}{2\alpha}} - (1-m)\epsilon^{-\frac{tm}{2\alpha}} \}, \quad . \quad . \quad (7)$$

or

$$v = V \frac{\sin}{\cos} \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \left( \cos + \frac{1}{m'} \sin \right) \frac{tm'}{2\alpha}, \quad . \quad . \quad . \quad (8)$$

where

$$m = \sqrt{1-4i^2\pi^2 \frac{\alpha}{\beta}}, \quad m' = \sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1};$$

and

$$\frac{dQ}{dt} = C\epsilon^{-\frac{t}{\alpha}} + \frac{Vi\pi}{kl} \cos \frac{i\pi x}{l} \cdot \frac{\epsilon^{-\frac{t}{2\alpha}}}{m} (\epsilon^{\frac{tm}{2\alpha}} - \epsilon^{-\frac{tm}{2\alpha}}), \quad . \quad . \quad . \quad (9)$$

or

$$\frac{dQ}{dt} = C\epsilon^{-\frac{t}{\alpha}} + \frac{Vi\pi}{kl} \cdot \cos \frac{i\pi x}{l} \cdot \frac{2\epsilon^{-\frac{t}{2\alpha}}}{m'} \cdot \sin \frac{tm'}{2\alpha}. \quad . \quad . \quad . \quad (10)$$

In the intermediate case, when  $m = m' = 0$ ,

$$v = V \frac{\sin \frac{i\pi x}{l}}{\cos \frac{i\pi x}{l}} \epsilon^{-\frac{t}{2\alpha}} \left(1 + \frac{t}{2\alpha}\right), \quad . \quad . \quad . \quad (11)$$

and

$$\frac{dQ}{dt} = C \epsilon^{-\frac{t}{\alpha}} + \frac{Vi\pi}{kl} \cos \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \cdot \frac{t}{\alpha} \quad . \quad . \quad (12)$$

The current  $C \epsilon^{-\frac{t}{\alpha}}$  does not influence the potential in any way. The above solutions suppose that the initial current is  $C$ , and the initial potential  $v = V \frac{\sin \frac{i\pi x}{l}}{\cos \frac{i\pi x}{l}}$ , and give the potential and current at any time after. When  $\sin \frac{i\pi x}{l}$  is taken, the potential at the ends of the wire is always zero; and when  $\cos \frac{i\pi x}{l}$  is taken, the current is always zero at the ends.

After this preliminary we can pass to more practical cases. In the first place, let a constant current  $\frac{V}{kl}$  be flowing through the wire, caused by a battery of negligible resistance and electromotive force  $V$ ; and let the potential of the wire be  $V\left(1 - \frac{x}{l}\right)$ , so that it is  $V$  at the end  $P$  and  $0$  at the end  $Q$ . By Fourier's theorem,

$$V\left(1 - \frac{x}{l}\right) = \frac{2V}{\pi} \sum_1 \frac{1}{i} \sin \frac{i\pi x}{l};$$

therefore, if the end  $P$  is put to earth at the time  $t = 0$ , the potential at the time  $t$  is

$$\begin{aligned} v = & \frac{2V}{\pi} \sum \frac{1}{i} \sin \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \cdot \left\{ \frac{1+m_i}{2m_i} \cdot \epsilon^{\frac{tm_i}{2\alpha}} - \frac{1-m_i}{2m_i} \cdot \epsilon^{-\frac{tm_i}{2\alpha}} \right\} \\ & + \frac{2V}{\pi} \sum \frac{1}{i} \sin \frac{i\pi x}{l} \cdot \epsilon^{-\frac{t}{2\alpha}} \left( \cos + \frac{1}{m'_i} \sin \right) \frac{tm'_i}{2\alpha} \quad . \quad (13) \end{aligned}$$

by (7) and (8), where the first series includes all integral values of  $i$  which make  $4i^2\pi^2\frac{\alpha}{\beta} - 1$  negative, and the second series all the rest up to  $i = \infty$ . And by (9) and (10),

$$\begin{aligned} \frac{dQ}{dt} = & \frac{V}{kl} \cdot \epsilon^{-\frac{t}{\alpha}} - \frac{2V}{kl} \sum \cos \frac{i\pi x}{l} \cdot \frac{\epsilon^{-\frac{t}{\alpha}}}{m_i} \left( \epsilon^{\frac{tm_i}{2\alpha}} - \epsilon^{-\frac{tm_i}{2\alpha}} \right) \\ & - \frac{2V}{kl} \sum \cos \frac{i\pi x}{l} \cdot \frac{2\epsilon^{-\frac{t}{2\alpha}}}{m'_i} \cdot \sin \frac{tm'_i}{2\alpha} \quad . \quad (14) \end{aligned}$$

expresses the current at time  $t$ . If the wire is originally everywhere at potential zero and without current, the potential  $v'$  and current  $\frac{dQ'}{dt}$  at time  $t$  after the end P is raised to potential V, the end Q being to earth, are

$$v' = V \left( 1 - \frac{x}{l} \right) - v,$$

$$\frac{dQ'}{dt} = \frac{V}{kl} - \frac{dQ}{dt},$$

where  $v$  and  $\frac{dQ}{dt}$  have the values given in (13) and (14).

Suppose  $4\pi^2 \frac{\alpha}{\beta} > 1$ , then the first series in (13) and (14) disappear, and we have

$$v = \frac{2V}{\pi} \cdot \epsilon^{-\frac{t}{2\alpha}} \sum_1^{\infty} \frac{1}{i} \sin \frac{i\pi x}{l} \left( \cos + \frac{1}{m'_i} \sin \right) \frac{tm'_i}{2\alpha}, \quad (15)$$

$$\frac{dQ}{dt} = \frac{V}{kl} \cdot \epsilon^{-\frac{t}{\alpha}} - \frac{4V}{kl} \cdot \epsilon^{-\frac{t}{2\alpha}} \sum_1^{\infty} \frac{1}{m'_i} \cos \frac{i\pi x}{l} \cdot \sin \frac{tm'_i}{2\alpha}. \quad (16)$$

The extra current is exhibited in (16) as consisting of two parts. One, a current  $\frac{V}{kl} \cdot \epsilon^{-\frac{t}{\alpha}}$ , uniform at all parts of the wire, which dies away without oscillations with a rapidity proportional to  $\frac{1}{\alpha}$ . This current is due entirely to the momentum of the original current  $\frac{V}{kl}$ . The other part,

$$- \frac{4V}{kl} \cdot \epsilon^{-\frac{t}{2\alpha}} \sum_1^{\infty} \frac{1}{m'_i} \cos \frac{i\pi x}{l} \cdot \sin \frac{tm'_i}{2\alpha},$$

is due entirely to the original charge of the wire, and consists at any point  $x$  of an infinite series of currents alternately positive and negative, which die away with only half the rapidity. These oscillations are of greatest intensity at the end P, and least at the end Q. They are insensible both when  $\frac{\alpha}{\beta}$  is very small and when it is very large. In the former case only the higher terms in (13) and (14) are periodic with respect to the time; and in the latter case they become very rapid and weak in the same proportion. But when the time-constants  $\alpha$  and  $\beta$  are not very different, the oscillations are of considerable strength, and may become observable by proper means. Sup-

pose  $\frac{\alpha}{\beta}$  of such magnitude that  $\sqrt{4i^2\pi^2 \frac{\alpha}{\beta} - 1}$  is appreciably  $= 2i\pi\sqrt{\frac{\alpha}{\beta}}$ , then the time of a complete oscillation, including a positive and a negative current at any point, is nearly  $2\sqrt{\alpha\beta}$ , so that there are  $\sqrt{\frac{\alpha}{\beta}}$  complete oscillations in the time  $2\alpha$ .

The strength of these oscillations is proportional to  $\sqrt{\frac{\beta}{\alpha}}$ ; so that the larger  $\frac{\alpha}{\beta}$  the weaker the oscillations, they being at the same time more rapid in the same proportion.

The time-integral of the extra current is

$$\frac{V\alpha}{kl} - \frac{Vcl}{2} \left( \frac{x^2}{l^2} - \frac{2x}{l} + \frac{2}{3} \right),$$

where the first part is the same at all points, and is due entirely to the momentum of the initial current. The second part is the excess of the positive over the negative currents due to the initial charge, and is twice as great at the end P as at Q. This is the same when  $s=0$ , or when there is no self-induction.

The work done in the wire by the extra current is

$$\int_0^\infty kl \left( \frac{dQ}{dt} \right)^2 dt,$$

when  $\frac{dQ}{dt}$  is the same at all points, and

$$\int_0^l \int_0^\infty k \left( \frac{dQ}{dt} \right)^2 dx dt$$

when  $\frac{dQ}{dt}$  varies with  $x$ . Hence the amount of work done by the first part of the current in equation (16) is  $\frac{sl}{2} \times \left( \frac{V}{kl} \right)^2$ , and by the second part  $\frac{V^2 cl}{6}$ , which was the energy of the initial charge  $= \frac{1}{2} \int_0^l Vc \left( 1 - \frac{x}{l} \right)^2 dx$ .

As another example, suppose that before the time  $t=0$  a uniform current  $\frac{V}{kl}$  existed in the wire, with potential  $v = V \left( 1 - \frac{x}{l} \right)$ , and that at the time  $t=0$  both ends of the wire are instantaneously and simultaneously insulated without allow-



ing a spark to pass. Then we have  $\frac{dQ}{dt}=0$  at P and Q. Let us first consider  $v$  and  $\frac{dQ}{dt}$  resulting from the initial charge, supposing  $4\pi^2\frac{\alpha}{\beta}>1$ . By Fourier's theorem,

$$V\left(1-\frac{x}{l}\right)=\frac{V}{2}+\frac{2V}{\pi^2}\sum_1\frac{1-\cos i\pi}{i^2}\cos\frac{i\pi x}{l},$$

where  $\frac{V}{2}$  is the final potential. Therefore, by (8), that part of the potential due to the initial charge is

$$\frac{V}{2}+\frac{2V}{\pi^2}\epsilon^{-\frac{t}{2a}}\sum_1\frac{1-\cos i\pi}{i^2}\cdot\cos\frac{i\pi x}{l}\left(\cos+\frac{1}{m'_i}\sin\right)\frac{tm'_i}{2a}; \quad (17)$$

and by (10) that part of the current due to the initial charge is

$$\frac{2V}{\pi kl}\epsilon^{-\frac{t}{2a}}\sum_1\frac{1-\cos i\pi}{i}\cdot\sin\frac{i\pi x}{l}\cdot\frac{2}{m'_i}\cdot\sin\frac{tm'_i}{2a}. \quad (18)$$

To find the potential and current due to the initial current, we have

$$\frac{V}{kl}=\frac{2V}{\pi kl}\sum_1\frac{1-\cos i\pi}{i}\sin\frac{i\pi x}{l};$$

therefore

$$\frac{dQ}{dt}=\frac{2V}{\pi kl}\cdot\epsilon^{-\frac{t}{2a}}\sum_1\frac{1-\cos i\pi}{i}\sin\frac{i\pi x}{l}(A_i\cos+B_i\sin)\frac{tm'_i}{2a},$$

and

$$v=V'+\frac{2V}{\pi^2}\sum_1\frac{1-\cos i\pi}{i^2}\cos\frac{i\pi x}{l}\epsilon^{-\frac{t}{2a}}\left(\frac{A_i}{2}\cos+\frac{B_i}{2}\sin\right. \\ \left.+\frac{B_im'_i}{2}\cos-\frac{A_im'_i}{2}\sin\right)\frac{tm'_i}{2a},$$

where  $V'$ ,  $A_i$ , and  $B_i$  are constants. The conditions to determine them are that  $v=0$  when  $t=0$  and when  $t=\infty$ . Also  $\frac{dQ}{dt}=\frac{V}{kl}$  when  $t=0$ . Therefore

$$V'=0, \quad A_i=1, \quad \text{and} \quad B_i=-\frac{1}{m'_i}.$$

Thus

$$\frac{dQ}{dt}=\frac{2V}{\pi kl}\cdot\epsilon^{-\frac{t}{2a}}\sum_1\frac{1-\cos i\pi}{i}\sin\frac{i\pi x}{l}\cdot\left(\cos-\frac{1}{m'_i}\sin\right)\frac{tm'_i}{2a}, \quad (19)$$

$$v=-4V\cdot\frac{\alpha}{\beta}\cdot\epsilon^{-\frac{t}{2a}}\sum\frac{1-\cos i\pi}{m'_i}\cos\frac{i\pi x}{l}\cdot\sin\frac{tm'_i}{2a}. \quad (20)$$

The actual potential is the sum of (17) and (20), and the actual current the sum of (18) and (19). When  $\frac{\alpha}{\beta}$  is large the initial

charge may be neglected altogether. Considering only the potential and current due to the initial current, we find that the current in the wire consists of a series of decreasing waves in opposite directions, causing corresponding changes in the potential of the wire. At the first moment after disconnection

the potential of the end Q becomes positive  $= V \sqrt{\frac{\alpha}{\beta}}$  nearly,

and the end P negative to an equal extent. Provided this electromotive force suddenly developed is not sufficiently great to cause a spark, this state of things is rapidly reversed, the end P becoming positive and the end Q negative, which is followed by another reversal, and so on till the energy of the initial current is all used up against the resistance of the wire.

It is obvious that the simplicity (?) of the above formulæ must be considerably departed from in all practical cases that occur, as in the above  $c$  and  $s$  are assumed to be the same for every unit of length of the wire, which cannot be true, except perhaps in a coiled submarine cable. But we may be sure that, in virtue of that property of the electric current which Professor Maxwell terms its "electromagnetic momentum," whenever any sudden change of current or of charge takes place in a circuit possessing an appreciable amount of self-induction, the new state of equilibrium is arrived at through a series of oscillations in the strength of the current which may be noticeable under certain circumstances. It is naturally difficult to observe such oscillations with a galvanometer; but some telegraph-instruments show them very distinctly. For instance, there is Wheatstone's "alphabetical indicator." The pointer of this instrument is moved one letter forward round a dial by every current passing through it, provided the currents are alternately positive and negative. Now if an insulated straight wire a few miles in length is suddenly raised to a high potential by means of a single current of very short duration from a magneto-electric machine, and then immediately discharged to earth through the coils of an "indicator," the pointer does not merely move one step forward, as it would if the discharge consisted of a single current, but several steps, indicating a succession of reverse currents. The same thing occurs when a condenser of small capacity is first charged to a high potential and then discharged through the instrument. Expressed in popular language, what happens is as follows. The first discharge-current is first retarded by the self-induction of the

coils, and then, acquiring momentum, carries to earth a greater quantity of electricity than the line or condenser originally contained, thus reversing the potential of the line. Hence a reverse current follows to restore the equilibrium, which in its turn carries to the line more than enough electricity to supply the deficiency; hence another current from line to earth; and so on, till the currents are too weak to produce any observable effect.

By supposing that the current at any moment is of the same strength in all parts of the coil, the theory of these alternating currents when a charged condenser is discharged through the coil is much simplified. Let  $Q_0$  be the initial charge and  $V$  the initial potential of the condenser, whose capacity is  $c$ ; and let  $R$  be the resistance and  $L$  the coefficient of self-induction of the coil. Then, if  $Q$  is the charge and  $v$  the potential of the condenser at the time  $t$ , the current in the coil is

$$\frac{dQ}{dt} = -c \frac{dv}{dt},$$

and

$$v = R \frac{dQ}{dt} + L \frac{d^2Q}{dt^2},$$

since  $v$  is the difference of potential between the ends of the coil. Therefore

$$cL \frac{d^2v}{dt^2} + cR \frac{dv}{dt} + v = 0,$$

the solution of which satisfying the conditions  $v = V$  when  $t = 0$  (and  $\frac{dv}{dt} = 0$  when  $t = 0$ ) is

$$v = \frac{V \epsilon^{-\frac{t}{2\alpha}}}{2 \sqrt{1 - 4 \frac{\alpha}{\beta}}} \left\{ \left( 1 + \sqrt{1 - 4 \frac{\alpha}{\beta}} \right) \epsilon^{\frac{t}{2\alpha} \sqrt{1 - 4 \frac{\alpha}{\beta}}} - \left( 1 - \sqrt{1 - 4 \frac{\alpha}{\beta}} \right) \epsilon^{-\frac{t}{2\alpha} \sqrt{1 - 4 \frac{\alpha}{\beta}}} \right\},$$

or

$$v = V \epsilon^{-\frac{t}{2\alpha}} \left( \cos + \frac{1}{\sqrt{4 \frac{\alpha}{\beta} - 1}} \sin \right) \frac{t}{2\alpha} \sqrt{4 \frac{\alpha}{\beta} - 1},$$

according as  $1 - 4 \frac{\alpha}{\beta}$  is + or -. And the current in the coil is

$$\frac{dQ}{dt} = \frac{V}{R} \cdot \frac{\epsilon^{-\frac{t}{2\alpha}}}{\sqrt{1 - 4 \frac{\alpha}{\beta}}} \left( \epsilon^{\frac{t}{2\alpha} \sqrt{1 - 4 \frac{\alpha}{\beta}}} - \epsilon^{-\frac{t}{2\alpha} \sqrt{1 - 4 \frac{\alpha}{\beta}}} \right),$$

or

$$\frac{dQ}{dt} = \frac{2V}{R} \cdot \frac{\epsilon^{-\frac{t}{2\alpha}}}{\sqrt{4\frac{\alpha}{\beta}-1}} \sin \frac{t}{2\alpha} \sqrt{4\frac{\alpha}{\beta}-1},$$

where  $\alpha = \frac{L}{R}$  and  $\beta = cR$ . In the first case, when  $1 > 4\frac{\alpha}{\beta}$ , the potential and current are never reversed; but in the second case, when  $4\frac{\alpha}{\beta} > 1$ , they are reversed an infinite number of times, the successive charges of the condenser decreasing in geometrical proportion. The current changes sign when  $t$  is any multiple of  $\frac{2\alpha\pi}{\sqrt{4\frac{\alpha}{\beta}-1}}$ , and has its maxima or minima

values when

$$\cos \frac{t}{2\alpha} \sqrt{4\frac{\alpha}{\beta}-1} = \frac{1}{2} \sqrt{\frac{\beta}{\alpha}}.$$

The quantity  $Q'$  of electricity conveyed in the first current is

$$Q' = Q_0 \left( 1 + \epsilon^{-\frac{\pi}{\sqrt{4\frac{\alpha}{\beta}-1}}} \right),$$

where  $Q_0$  is the initial charge of the condenser. As  $\frac{\alpha}{\beta}$  is increased  $Q'$  approaches  $2Q_0$  as its limit; *i. e.* when the resistance of the coil is reduced, or its magnetic capacity increased, the quantity of electricity conveyed by any current increases till it is nearly double the charge of the condenser at the commencement of that current, and the oscillations are more slowly diminished. The amount of energy expended by the first current is

$$\frac{V^2 c}{2} \left( 1 - \epsilon^{-\frac{2\pi}{\sqrt{4\frac{\alpha}{\beta}-1}}} \right),$$

where  $\frac{V^2 c}{2}$  is the energy of the original charge  $Q_0$ , which becomes indefinitely small as  $\frac{\alpha}{\beta}$  increases. The integral current, irrespective of sign, is

$$\frac{Q_0}{1 - \epsilon^{-\frac{\pi}{\sqrt{4\frac{\alpha}{\beta}-1}}}},$$

which increases indefinitely with  $\frac{\alpha}{\beta}$ . From the number of

oscillations in a given time,  $L$  may be determined in terms of  $R$  and  $c$ ; for if the current is reversed  $n$  times per second, then

$$L = \frac{1}{2c\pi^2 n^2} (1 + \sqrt{1 - c^2 R^2 \pi^2 n^2}).$$

Electrical vibrations due to induction occur under various circumstances. For example, the "false discharge" from a submarine cable; the oscillatory phenomena described by M. Blaserna and others; and Mr. Edison's "ætheric-force" experiments.

## XX. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 71.]

Feb. 10, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

**T**HE following papers were read:—

"On Repulsion resulting from Radiation."—Part III. By William Crookes, F.R.S. &c.

This paper contains an account of experiments on the action of radiation on bodies the surfaces of which have their radiating and absorbing powers modified by various coatings. The difference between a white and a lampblackened surface in this respect was at first not very decided; and experiments have been instituted with the object of clearing up some anomalies observed in the actions. Two pith disks, one white and the other black, were suspended on a light arm in a glass bulb by means of a fine silk fibre; after perfect exhaustion the white and black disks were found to be equally repelled by heat of low intensity, such as from the fingers, warm water, &c. A copper ball was then tried at gradually increasing temperatures. Up to  $250^{\circ}$  C. it repelled both equally, above that the black was more repelled than the white, and at a full red heat the repulsion of the black disk was very energetic. A lighted candle acts with more energy than the red-hot copper.

The presence of even a small quantity of aqueous vapour in the exhausted apparatus almost, if not quite, neutralizes the more energetic action which luminous rays appear to exert on a blackened surface.

After describing several different modifications and some new forms of apparatus devised to facilitate experiment, the author gives a drawing of an instrument which enables him to get quantitative measurements of the amount of incident light falling on it. It consists of a flat bar of pith, half black and half white, suspended horizontally in a bulb by means of a long silk fibre. A small magnet and reflecting-mirror are fastened to the pit; and a controlling magnet is fastened outside so that it can slide up and down the tube, and thus increase or diminish sensitiveness. The whole is completely exhausted, and then enclosed in a box

lined with black velvet, with apertures for the rays of light to pass in and out. A ray of light reflected from the mirror on to a graduated scale, shows the movements of the pith bar. The degrees of deflection produced by the light of a candle at distances from 6 feet to 35 feet are given.

The experimental observations and the numbers which are required by the theoretical diminution of light with the square of the distance are sufficiently close, as the following figures show:—

Candle	6 feet off	gives a deflection of	218°
"	12	"	54
"	18	"	24·5
"	24	"	13
"	10	"	77
"	20	"	19
"	30	"	8·5

The effect of two candles side by side is practically double, and of three candles three times that of one candle.

The action of various solid and liquid screens is next given.

A candle 3 feet off, giving a deflection of 180°, has its action reduced to the following amounts by

Yellow glass	161°
Blue	102
Green	101
Red	128
Water	47
Alum	27

A candle on each side of the apparatus, and equidistant from it, keeps the index ray of light at zero; by shading off either one or the other the light flies off to either extremity of the scale. This gives a ready means of balancing two sources of light one against the other. Thus, retaining the standard candle 48 inches off on the left of the bar, the index was brought to zero by placing on the right

2 candles	67 inches off,
1 candle behind solution of sulphate of copper	6 "
" " alum plate	14 "
A small gas-burner	113 " .

These experiments show how conveniently and accurately this instrument can be used as a photometer. By balancing a standard candle on one side against any source of light on the other, the value of the latter in terms of a candle is readily shown; thus in the last experiment the standard candle 48 inches off was balanced by a gas-flame 113 inches off. The lights were therefore in the proportion of  $48^2$  to  $113^2$ , or as 1 to  $5\frac{1}{2}$ . The gas-burner was therefore equal to  $5\frac{1}{2}$  candles.

By interposing screens of water or plates of alum, and so practically cutting off all the dark heat, the actual luminosity is measured. In addition to this, by interposing coloured glasses or solutions, any

desired colours can be measured either against the total radiation from a candle, its luminous rays, or any desired colour. One coloured ray can be balanced against another coloured ray by having differently coloured screens on either side.

The variations in the luminosity of a "standard" candle will cease to be of importance. Any candle may be taken; and if it be placed at such a distance from the apparatus that it will give a uniform deflection, say of 100 divisions, the standard can be reproduced at any subsequent time; and the burning of the candle may be tested during the photometric experiments by taking the deflection it causes from time to time, and altering its distance, if needed, to keep the deflection at 100 divisions.

If the pith bar in this instrument be blacked on alternate halves, an impetus given by a ray of light always acts in the same direction of movement. A candle causes it to spin round very rapidly until the suspending fibre is twisted up, and the rotation is stopped by the accumulated torsion.

By arranging the apparatus so that the black and white surfaces are suspended on a pivot instead of by a silk fibre, the interfering action of torsion is removed, and the instrument will rotate continuously under the influence of radiation. To this instrument the author has given the name of the "Radiometer," or "Light-Mill." It consists of four arms of very fine glass, supported in the centre by a needle-point, and having at the extremities thin disks of pith lampblackened on one side, the black surfaces all facing the same way. The needle stands in a glass cup; and the arms and disks are delicately balanced, so as to revolve with the slightest impetus.

In the 'Proceedings of the Royal Society' for 1875 (vol. xxiii. p. 373), the author gave a brief account of some of the earlier experiments with these instruments. In the present paper he enters very fully into the various phenomena presented by them, and gives Tables showing the number of revolutions made by the radiometer when exposed to a constant source of light removed different distances from the instrument. The law is that the rapidity of revolution is inversely as the square of the distance between the light and the instrument.

When exposed to different numbers of candles at the same distance off, the number of revolutions in a given time is in proportion to the number of candles, two candles giving twice the rapidity of one candle, and three giving three times, &c.

The position of the light in the horizontal plane of the instrument is of no consequence, provided the distance is not altered; thus two candles one foot off give the same number of revolutions per second, whether they are side by side or opposite to each other. From this it follows that if the radiometer is brought into a uniformly lighted space it will continue to revolve. This is proved to be the case by experiment.

The speed with which a sensitive radiometer will revolve in full sunshine is almost incredible; nothing is apparent but an undefined nebulous ring, which becomes at times almost invisible,

The number of revolutions per second cannot be counted: but it must be several hundreds; for one candle will make the arms spin round forty times a second.

The action of dark heat (*i. e.* from boiling water) is to repel each surface equally, and the movement of the radiometer is therefore arrested if a flask of boiling water is brought near it. The same effect is produced by ice.

From some observations made by the author, it appears probable that heat of a still lower refrangibility repels the white more than it does the black surface. Many instances are given of the radiometer revolving the reverse way. Thus breathing gently on the instrument will generally cause this effect to be produced.

An experiment is described with a radiometer the moving parts of which are of aluminium blacked on one side. When exposed to the radiation from a candle the arms revolve the normal way. On removing the candle they revolve the reverse way. Heated with a Bunsen burner the arms revolve the normal way as they are getting hot; but as soon as the source of heat is removed and cooling commences, rotation sets up in the reverse way, and continues with great energy till the whole is cold. The reverse movement during the cooling is apparently equal in energy to the normal movement as it is being heated.

It is easy to get rotation in a radiometer without having the surfaces of the disks differently coloured. An experiment is described with one having the pith disks blacked on both sides. On bringing a candle near it, and shading the light from one side, rapid rotation is produced, which is at once altered in direction by moving the shade to the other side.

The author describes many forms of radiometer, by means of which the movements can be exhibited to a large audience, or can be made to record themselves telegraphically on a self-recording instrument.

“On Repulsion resulting from Radiation.”—Part IV. By William Crookes, F.R.S. &c.

In this paper the author describes experiments on the repulsion produced by the different rays of the solar spectrum. The apparatus employed is the horizontal beam suspended by a glass fibre and having square pieces of pith at each end coated with lamp-black. The whole is fitted up and hermetically sealed in glass, and connected with an improved mercury pump. In front of the square of pith at one end a quartz window is cemented to the apparatus; and the movements of the beam, when radiation falls on the pith, are observed by a reflected ray of light on a millimetre-scale. The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected in a constant direction a beam of sunlight, which was received on an appropriate arrangement of slit, lenses, and prisms for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given



in the paper graphically as a curve, the maximum being in the ultra-red, and the minimum in the ultra-violet. Taking the maximum at 100, the following are the mechanical values of the different colours of the spectrum :—

Ultra-red . . . . .	100
Extreme red . . . . .	85
Red . . . . .	73
Orange . . . . .	66
Yellow . . . . .	57
Green . . . . .	41
Blue . . . . .	22
Indigo . . . . .	8½
Violet . . . . .	6
Ultra-violet . . . . .	5

A comparison of these figures with those usually given in text-books to represent the distribution of heat in the spectrum is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The author discusses the question, "Is the effect due to heat or to light?" There is no real difference between heat and light; all we can take account of is difference of wave-length; and a ray of a definite refrangibility cannot be split up into two rays, one being heat and one light. Take, for instance, a ray of definite refrangibility in the red. Falling on a thermometer it shows the action of heat; on a thermopile it produces an electric current; to the eye it appears as light and colour; on a photographic plate it causes chemical action; and on the suspended pith it causes motion. But all these actions are inseparable attributes of the ray of that particular wave-length, and are not evidence of separate identities.

The author enters into some theoretical explanations of the action of the different parts of the spectrum; but these cannot well be given in abstract.

An experiment is described by which sunlight was filtered through alum, glass, and water screens, so as to cut off the whole of the ultra-red or dark-heat rays. The ray of light which was thus freed from dark heat was allowed to fall on the pith surface of the torsion-apparatus, when it produced a deflection of 105°. On interposing a solution of iodine in disulphide of carbon the deflection fell to 2°, showing that the previous action was almost entirely due to *light*. With a candle tried under the same circumstances, the light filtered from dark heat produced a deflection of 37°, which was reduced to 5° by interposing the opaque solution of iodine.

In order to obtain comparative results among disks of pith coated with lampblack and with other substances, a torsion-apparatus was constructed in which two or more disks could be exposed one after the other to a standard light. One disk always being lampblackened pith, the other disks could be changed so as to get comparisons of action. If the action of radiation from a candle

on the lampblackened disk be taken as 100, the following are the proportions obtained :—

On Lampblackened pith .....	100
Iodide of palladium .....	87·3
Precipitated silver .....	56
Amorphous phosphorus .....	40
Sulphate of baryta .....	37
Milk of sulphur .....	31
Red oxide of iron .....	28
Scarlet iodide of mercury and copper	22
Lampblackened silver .....	18
White pith .....	18
Carbonate of lead .....	13
Rock-salt .....	6·5
Glass .....	6·5

In consequence of some experiments tried by Profs. Tait and Dewar, and published in 'Nature,' July 15, 1875, the author fitted up a very sensitive apparatus for the purpose of carefully examining the action of radiation on alum, rock-salt, and glass. The source of radiation was a candle. Perfectly transparent and highly polished plates of the same size were used; and the deflection was made evident by an index ray of light. Taking the action on the alum at 100, that on the rock-salt in five successive experiments was 81, 77·3, 71, 62·5, 60·4. This increasing action on the alum was found to be caused by efflorescence, which took place rapidly in the vacuum, and rendered the crystal partially opaque. A fresh alum plate being taken, this and the rock-salt were coated with lampblack and replaced in the apparatus, the black side away from the source of radiation, so that the radiation would pass through the crystal before reaching the lampblack. The action of radiation was in the proportion of blacked alum 100 to blacked rock-salt 73.

Rock-salt and glass were next tested against each other *in vacuo* in a torsion-balance. Professors Dewar and Tait say that rock-salt is inactive when the beam from a candle is thrown on it, while a glass disk is active. The author has failed to corroborate these results; he found the mean of several concordant observations to be—rock-salt 39, glass 40.

#### *The Measurement of the Force.*

The author describes a torsion-balance in which he is enabled to weigh the force of radiation from a candle, and give it in decimals of a grain. The principle of the instrument is that of W. Ritchie's torsion-balance, described in the Philosophical Transactions for 1830. The construction is somewhat complicated, and cannot be well described without reference to the diagrams which accompany the original paper. A light beam, having two square inches of pith at one end, is balanced on a very fine fibre of glass stretched horizontally in a tube, one end of the fibre being connected with a torsion-handle passing through the tube, and indicating angular

movements on a graduated circle. The beam is cemented to the torsion-fibre, and the whole is enclosed in glass and connected with the mercury pump and exhausted as perfectly as possible. A weight of 0.01 grain is so arranged that it can be placed on the pith or removed from it at pleasure. A ray of light from a lamp reflected from a mirror in the centre of the beam to a millimetre-scale 4 feet off shows the slightest movement. When the reflected ray points to zero, a turn of the torsion-handle in one or the other direction will raise or depress the pith end of the beam, and thus cause the index ray to travel along the scale to the right or to the left. If a small weight is placed on one end so as to depress it, and the torsion-handle is then turned, the tendency of the glass fibre to untwist itself will ultimately balance the downward pressure of the weight, and will again bring the index ray to zero. It was found that when the weight of the  $\frac{1}{100}$  of a grain was placed on the pith surface, the torsion-handle had to be turned 27 revolutions and  $353^\circ$ , or  $10073^\circ$  before the beam became horizontal. The downward pressure of the  $\frac{1}{100}$  of a grain was therefore equivalent to the force of torsion of the glass thread when twisted through  $10073^\circ$ .

The author next ascertained what was the smallest amount of weight which the balance would indicate. He found that  $1^\circ$  of torsion gave a very decided movement of the index ray of light, a torsion of  $10073^\circ$  balancing the  $\frac{1}{100}$  of a grain, while  $10074^\circ$  overbalanced it. The balance will therefore turn to  $\frac{99}{100,000,000}$  of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed.

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0.000444 grain; of a candle 6 inches off 0.001772 grain. At half the distance the weight of radiation should be four times, or 0.001776 grain; the difference between theory and experiment being only four millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described by the author, follow rigidly the law of inverse squares. An examination of the differences between the separate observations and the mean shows that the author's estimate of the sensitiveness of his balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

One observation of the weight of sunlight is given; it was taken on December 13; but the sun was so obscured by thin clouds and haze that it was only equal to 10.2 candles 6 inches off. Calculating from this datum, it is seen that the pressure of sunshine is 2.3 tons per square mile.

The author promises further observations with this instrument, not only in photometry and in the repulsion caused by radiation, but in other branches of science in which the possession of a balance of such incredible delicacy is likely to furnish valuable results.

## GEOLOGICAL SOCIETY.

[Continued from p. 72.]

June 7, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the British Fossil Cretaceous Birds." By Harry Govier Seeley, Esq., F.L.S., F.G.S., Professor of Physical Geography in Bedford College, London.

2. "On two Chimæroid Jaws from the Lower Greensand of New Zealand." By E. T. Newton, Esq., F.G.S., of H. M. Geological Survey.

3. "On a Bone-bed in the Lower Coal-measures, with an enumeration of the Fish-remains of which it is principally composed." By J. W. Davis, Esq., F.L.S., F.G.S.

4. "Note on a species of Foraminifera from the Carboniferous formation of Sumatra." By M. Jules Huguenin.

5. "On the Triassic Rocks of Somerset and Devon." By W. A. E. Ussher, Esq., F.G.S.

The author stated that the Trias of Devon and Somerset was divisible into three groups, occupying distinct areas.

The first lies north of the Mendip Hills, where the Trias is thinnest and assumes its simplest characters, consisting of marls and Dolomitic conglomerate, the former predominating, the latter not only occurring as a basement series, but in some cases persisting continuously upwards as the marginal equivalent of the marl, as Rhætic beds overlies both alike. South of the Mendips the Trias is similarly constituted, but is of much greater thickness.

The second area embraces the country south of the Polden Hills as far as a north and south line through Taunton.

The chief portion of the Trias in this area, as in the northern, consists of marls; but unless the breccias of Wembdon, near Bridgewater, are portions of a basement series, faulted up, but elsewhere concealed, the lower division consists of sandstones found resting on the flanks of the Quantocks, flanking outliers of the older rocks, and here and there faulted up.

In the lower parts of the marls in the Vale of Taunton and other places occasional beds of sandstone are found, which from their position may be regarded as equivalent to the Upper Keuper Sandstone of Professor Hull. The sandstones of this area differ from the conglomerates of the Mendip country in occupying a definite subordinate horizon to the marls, and not dovetailing into them.

The third area, bounded on the north by the Bristol Channel, on the south by the English Channel, on the east by the Blackdown range, and on the west by the Culm and Devonian highlands, presents the most complex relations of the Trias in the south-western counties.

The upper member of the series, as in the other districts, consists of marls; they contain occasionally thin beds of sandstone towards the base, which may be considered, although apparently of local

occurrence, as Upper Keuper Sandstone. They are underlain by sandstones. In these two upper divisions we have the continuation of the whole Triassic series of the second area, and in the third underlying division conglomerate and pebble-beds. From Watchet to a point west of Wellington this division is represented by hard bedded conglomerate, thence to the south coast by sands with rounded grains and pebbles, at first of quartz grit and slate, gradually giving place to the large foreign-derived quartzites of Budleigh Salterton.

A second series of marls is found to underlie the conglomerate and pebble-beds conformably. This attains as much as 500 feet in thickness near the south coast, but in the northern part of the district does not appear to exceed 200 feet.

The base of the series is composed of sandstone, breccia, and occasionally clays, occurring at different horizons in different parts of the area, and each variety locally predominant over the others.

The clay beds appear to be confined to the neighbourhoods of Exeter and Crediton. The upper part of the division south of Bradninch seems to consist of red sandstones, the lowermost and principal part of the division consisting of breccia. North of Bradninch the sandstone occupies the principal portion of the division, sometimes apparently to the exclusion of the breccia. Towards Wiveliscombe the upper part of the series for from 15 to 30 feet consists of breccia and breccia-conglomerate, the major part consisting as before of sandstones. North of Wiveliscombe breccia begins to prevail, and in the Stogumber valley principally represents the division. Owing to the conformity of the various members of this latter group, they appear to represent continuous deposition. We might fairly consider the upper marls and sandstones as representing the Keuper, as their average thickness taken together can scarcely be under 1000 feet.

The lowermost sandstone and breccia may be taken as equivalents of part of the Bunter, as they are probably 1000 feet thick near the south coast, and 300 where least developed.

The representation of the Muschelkalk would lie then between the conglomerate and Lower Marl divisions, and the one rejected be incorporated with Keuper or Bunter.

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## *XXI. Intelligence and Miscellaneous Articles.*

REPORT ON SOME EXPERIMENTS OF DR. E. ROOT, OF BOSTON,  
CONCERNING THE PENETRATION OF PLATINUM BY ELECTRO-  
LYTIC GASES. BY DR. HELMHOLTZ.

THE experiments which I related to the Academy on the 21st July, 1873, had led me to consider that, in galvanic polarization, portions of the gases, not merely adhering to the surface, but also penetrating deeper into the platinum, must play a part, of which the possibility had already been indicated by Graham's experiments on palladium and platinum. In order to obtain a practical demonstration of the penetration of platinum by the gases in galvanic polarization, I induced Dr. Elihu Root to ascertain, by experiments

in the physical laboratory of the Berlin University, whether the hydrogen carried by electrolysis to one side of a thin plate of platinum would after a time make itself perceptible on the opposite side, by producing galvanic polarization there also. These experiments have in fact given the result expected.

To the two opposite sides of a plate of platinum 0.02 millim. thick, and placed vertical, the even edges of two glass vessels, the form of which was that of tubulated receivers of a retort, were cemented with sealing-wax. One of the openings of each was cemented to the above-mentioned platinum plate; the second was directed upwards. The edges of the platinum plate projected on all sides outward beyond the layer of the cement, so that certainly no conducting bridge of fluid existed between one side and the other of the platinum. The glass vessels were filled with distilled water to which a few drops of pure sulphuric acid had been added; through the upper openings of the vessels two other platinum plates dipped into it. Previously to the putting-together of the apparatus the three platinum plates were cleaned by heating and washing. In order to prove whether the platinum was perfectly compact, a sample of the sheet platinum used was fused as a closure to the extremity of one branch of a manometer exhausted of air; it was found that in the course of two months no perceptible trace of air had penetrated.

Since the travelling of gas in platinum can at any rate only take place with extreme slowness, and the small quantity which has penetrated to the further surface can be readily exhausted, the entire apparatus was kept under the exhausted receiver of an air pump, so that only the conducting-wires to the three platinum plates extended outside. Further, it was necessary to avoid permitting a depolarizing current, even of short duration, to be generated. On this account, instead of the galvanometer an instrument had to be employed which could indicate the existing difference of potential without a lasting current. For this purpose Lippmann's capillary electrometer\* proved very suitable. In the observations the microscope was kept directed to a definite place in the capillary glass tube, and the difference of pressure was determined which was necessary in order, on alternating connexion of the two poles of the electrometer with the two platinum plates, to carry the mercury thread in the capillary tube to the same division-stroke. In the following a positive pressure-difference indicates a greater positive potential in plate B than in plate C; the difference 1 corresponds to that of about  $\frac{1}{300}$  of a Daniell element.

The apparatus having been put together, the three platinum plates were left for 18 hours in conducting connexion with each other and with the earth, in order to get rid of the remains of older polarizations. They were then insulated from each other and from the earth; and during 14 days the electromotive force between the central one (B) and one of the outer ones (C) was investigated daily, to ascertain if from other causes rapid variations of polari-

\* Poggendorff's *Annalen*, vol. cxlix. p. 551.

zation were to be expected. The force fell, during the first half of this time; from 4.83 to 0.40, and then gradually rose to 1.37, to sink again toward the close of the period mentioned; but the alterations took place slowly and without any quick leaps.

It now appeared that if for only five minutes two Daniell elements were closed between the one outer plate A and the middle plate B, which carried hydrogen to the plate B, a change occurred in the behaviour of the other side of B toward the second outer plate C.

For example, in a series of experiments, immediately before closing the current through A and B the pressure-difference between B and C had been  $-0.6$ ; after the current had been passing for five minutes, while plate C remained electrically insulated, the pressure-difference was  $+5.2$ ; it then rose in the next three hours, the plates being insulated, to 17.1; and 18 hours later it was again  $-3$ .

In other experiments the current between A and B was closed during 12 or 18 hours. The difference of potential between B and C was then greatest immediately after the interruption of that current, and gradually diminished during the hours that followed.

When the current between A and B received the opposite direction, so that it urged oxygen toward B, the opposite result occurred on the other side of B, with the same quickness, and partly with same, partly with greater intensity than in the previous hydrogen polarization. Whether this difference in the quantity of the effect is or is not conditioned by the antecedent hydrogen polarization cannot be decided from the experiments hitherto made. As sometimes the state of the plates B and C has been equalized by joining them to the circuit, a sensible quantity of hydrogen may then have been also carried over to C, and thence may arise the more powerful action of B, polarized with oxygen. In general, quantitative determinations of the action were frustrated by the circumstance that the gas once forced into the plate only very slowly becomes equally diffused in its interior, and can only be removed again very slowly. Moreover the action is quite evident when only a single Daniell element between A and B is employed; and, on the other hand, the action of two Bunsen elements was but a trifle greater than that of two Daniells. The cause of this may be, that when electrolytic gas-production commences, the escaping portions of the gases withdraw themselves from the force which presses them into the platinum; and hence, probably, a further reinforcement of the electromotive force augments the decomposition of the water, but the pressure of the gases into the platinum is increased very little or not at all—as indeed it is known that the polarization of the plates, when once it has arrived at gas-development, can be raised but little higher.

When plates B and C were put for a short time in conducting connexion after B had been charged with gas from A, immediately after interruption of the conduction the difference of potential between the two was  $=0$ , but then rose again on the same side as it had been before the conducting connexion, in precisely the same way as this usually takes place when both plates have been polarized by an electric current led directly through them.

This reappearance of a previous polarization I have already endeavoured to account for by the depolarizing current only removing the gases from the most superficial layer of the platinum, new supplies of gas pressing afterwards from the deeper parts to the surface. This takes place, therefore, in the same manner when the entire store of gas is derived from the other side.

Experiments made by Dr. Root, with a suitably modified apparatus, on the question whether free hydrogen in contact with the off side of B, while free oxygen was on the off side of C, would penetrate through the plates and produce polarization on the other side, did not give sufficiently explicit results. In these cases even the electrical force is wanting which urges the positive hydrogen molecules into the platinum.—*Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*, March 1876, pp. 217–220.

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#### ON ROSCOELITE—A VANADIUM MICA. BY F. A. GENTH.

I am indebted to Dr. James Blake, of San Francisco, California, for a small quantity of the very interesting mineral which he called "Roscoelite" in honour of Professor Roscoe, whose important investigations have put vanadium in its proper place among the elements.

Roscoelite occurs in small seams, varying in thickness from  $\frac{1}{10}$  to  $\frac{1}{10}$  of an inch, in a decomposed yellowish, brownish, or greenish rock. These seams are made up of small micaceous scales, sometimes  $\frac{1}{4}$  of an inch in length, mostly smaller, and frequently arranged in stellate or fan-shaped groups. They show an eminent basal cleavage. Soft. The specific gravity of the purest scales (showing less than one per cent. of impurities) was found to be 2.938; another specimen of less purity gave 2.921. Lustre pearly, inclining to submetallic. Colour dark clove-brown to greenish brown, sometimes dark brownish green.

Before the blowpipe it fuses easily to a black glass, colouring the flame slightly pink. With salt of phosphorus gives a skeleton of silicic acid, a dark yellow bead in the oxidizing flame, and an emerald-green bead in the reducing flame. Only slightly acted upon by acids, even by boiling concentrated sulphuric acid; but readily decomposed by dilute sulphuric acid when heated in a sealed tube at a temperature of about 180° C., leaving the silicic acid in the form of white pearly scales, and yielding a deep bluish-green solution. With sodic carbonate it fuses to a white mass. The Roscoelite which I received for investigation was so much mixed with other substances, such as gold, quartz, a feldspathic mineral, a dark mineral and very minute quantities of one of orange colour, that it was impossible to select for analysis material of perfect purity. For this reason I have delayed the publication of my results, which were obtained over one year ago, in the hope of being able to repeat my analyses with better and purer specimens; but I now give the results of my analyses because there is no prospect of getting any more of this mineral, as in a letter of Dr. Blake, dated San Francisco, April 5th, 1876, he says that the mine



in which it occurs cannot be worked any further until a tunnel has been run, and that it is quite uncertain when this will be done.

Although by no means perfect, my results approach the truth and give a fair idea of the composition of the mineral, even if the evident admixture of other minerals, varying in the different samples analyzed from about one to perhaps over twelve per cent., does not permit one to calculate the atomic ratio of the constituents and establish the constitution of this species. There is especially an uncertainty with reference to the quantities of silicic acid, alumina, and potassa which belong to the Roscoelite, or which may have been introduced by admixtures of feldspathic and other minerals, as will appear from the results given below, which show that the mineral, when decomposed with sulphuric or dilute hydrofluoric acid, generally gives only about six per cent. of potassa, while fusion with calcic carbonate and ammonic chloride yields from eight to nine per cent. Some of these uncertainties could have been removed if a larger quantity of the mineral had been at my disposal.

Particular attention was paid to the correct determination of the vanadium, and the form in which it exists in the Roscoelite.

The separation of vanadium is attended with great difficulties; and I have not found any of the methods of separation to give fully reliable results. This is in part owing to the incomplete precipitation of the vanadic acid, and in part to the impossibility of washing the precipitates completely without loss of vanadium. It was therefore always determined by the only method which I found to give fully reliable results—by titration with potassic permanganate.

After the separation from the other elements, the vanadic acid was reduced by hydrosulphuric acid into  $V_2O_4$ , which, after the excess of hydrosulphuric acid had been expelled by continued boiling, was reoxidized into  $V_2O_5$  by permanganate. I have satisfied myself by numerous experiments that, no matter whether only a very minute quantity of sulphuric acid is present, or a very large excess, the  $V_2O_4$  is completely oxidized into  $V_2O_5$  by this process.

For the determination of the state of oxidation of the vanadium in the Roscoelite, a quantity of the mineral was dissolved in dilute sulphuric acid in a sealed tube at a temperature of about  $180^\circ C.$ , and was titred after cooling; the liquid was then reduced by hydrosulphuric acid; and after boiling-off the excess of the latter, it was again titred. From the quantity of oxygen required for oxidation in both cases, it was found that vanadium in the mineral is present as  $V_6O_{11} = 2V_2O_3, V_2O_5$ .

The determinations of the other elements were made by the usual methods.

The finely powdered mineral was dried (unless otherwise stated) for two days over sulphuric acid; and the different samples gave the following results:—

*a* (purest scales). The analysis was made by dissolving one portion in sulphuric acid and determining in this the quantity and state of oxidation of the vanadium, the silicic acid, and insoluble impurities. The latter were left behind in dissolving the silicic acid in sodic carbonate and gave 0.85 per cent.; a second portion

was decomposed by sodic carbonate and nitrate, and a third for the determination of the alkalis by J. L. Smith's method. The  $V_6O_{11}$  given below is the mean of the two determinations. *b* (another sample, not quite as pure as *a*) was analyzed by fusion. *c*, still more contaminated with impurities, was analyzed by dissolving in dilute sulphuric acid in a sealed tube, &c.: *ca* is the result of this analysis; *cβ* after deducting 11·45 per cent. of impurities. *d* (another sample) was decomposed by dilute hydrofluoric acid; the analysis was unfortunately lost, excepting the determinations given below; the material for this analysis had not been dried over sulphuric acid. *e*. This sample was dried over sulphuric acid for several weeks; a portion, which was decomposed by sulphuric acid, gave 5·37 per cent. insoluble silicates, 0·23 per cent. of gold, and 43·24 per cent. of silicic acid; the  $V_6O_{11}$  was determined by difference. The results given below were obtained by decomposing the mineral by fusion.

	<i>a.</i>	<i>b.</i>	<i>ca.</i>	<i>cβ.</i>	<i>d.</i>	<i>e.</i>
Insoluble silicates, } quartz, gold, &c. }	[0·85]	.....	11·45	.....	8·91	[5·60]
SiO <sub>2</sub> .....	47·69	47·82	43·46	48·60	.....	46·81
Al <sub>2</sub> O <sub>3</sub> .....	14·10	12·60	10·52	11·76	.....	15·78
FeO .....	1·67	3·30	2·03	2·27	.....	1·58
MgO .....	2·00	2·43	1·74	1·95	.....	2·31
CaO .....	trace	trace	0·20	0·23	.....	trace
Na <sub>2</sub> O (trace Li <sub>2</sub> O) =	0·19	0·33	0·30	0·34	} 5·96	0·60
K <sub>2</sub> O .....	7·59	8·03	5·35	5·98		8·89
V <sub>6</sub> O <sub>11</sub> .....	22·02	21·36	20·50	22·92		20·16
Ignition .....	4·96	5·13	5·32	5·95	6·34	3·87
	100·22	101·00	100·87	100·00	.....	100·00

A mineral very similar in composition, and perhaps a compact impure variety of Roscoelite, is found associated with the scales. It has the appearance of a massive dark green chlorite, or that of some varieties of serpentine. The analysis was made by fusion &c., and gave:—

SiO <sub>2</sub> .....	= 46·09
Al <sub>2</sub> O <sub>3</sub> .....	= 17·46
FeO .....	= 1·95
MgO .....	= 2·18
Na <sub>2</sub> O .....	= 0·18
K <sub>2</sub> O .....	= 8·66
V <sub>6</sub> O <sub>11</sub> .....	= 17·53
Loss by ignition..	= 6·37
	100·42

Silliman's *American Journal*, July 1876.

#### ON HEAT-SPECTRA. BY M. AYMONNET.

M. Desains showed, in 1868:—(1) that if a body is heated to different temperatures, the increase of energy of its radiation affects both the luminous and the obscure portion; (2) that when the heat-spectra given by different sources are considered, the position of the maximum of intensity varies with the nature of the sources.

M. Lecoq de Boisbaudran, in 1871, pointed out that, in the spectrum of a metal, the higher the temperature of the source, the nearer is the brightest line to the ultra-violet; and he showed that a certain line in the vicinity of the infra-red, visible at one temperature, ceases to be so at a higher temperature, and that then some lines towards the ultra-violet become visible which at first were not so. MM. Brünner and Salet have remarked that hydrogen and a certain number of metalloids gave different spectra when the nature of the sources was different.

Having been permitted to take advantage of the resources of the laboratory under the direction of M. Desains at the Sorbonne, I proposed to myself (1) to determine the distribution of the heat in the calorific spectrum produced by a Bourbouze lamp and a refracting system of flint glass, (2) to study the variations of this spectrum with the temperature of the source, and (3) to observe also the absorption-spectra of various bodies, and their variations with the temperature of the source.

In all my experiments the apparatus preserved its initial position throughout.

The lamp employed is formed essentially of a Bunsen's burner surmounted by a cylinder of sheet platinum closed at the top. In this cylinder the combustion took place of illuminating-gas and of air supplied by a tromp, the tension of this air being measured by a manometer. To secure different fixed temperatures it was sufficient to vary the tension of the air arriving at the lamp, and to bring to the latter the quantity of gas requisite to give it the maximum of brightness, which always corresponds to its minimum of sonorousness. At that instant the combustion appears to take place in the cylinder only.

To estimate the various temperatures a thermoelectric actinometer is placed at a distance of 65 centims. from the lamp. The temperatures are not measured, but defined by the difference between the positions of equilibrium of the needle of the galvanometer when the actinometer receives and when it does not receive the heat of the source.

These different sources, viewed in the spectroscope with a prism, give continuous spectra.

In my experiments I have operated sometimes by the usual method of impulsions, sometimes by a different method (in which definite positions of equilibrium only are observed). I shall subsequently return to this new manner of operating. For the present I confine myself to indicating a few results.

I readily perceived at the commencement that the maximum approaches the least-refrangible part of the spectrum in proportion as the temperature of the source is lowered. I saw moreover that in this case the curves representing the results glide, without losing their form, in the same direction as the maximum.

I next divided the spectra to be studied into four portions, defined by the angular distances from the extreme red to their limits; and taking the ratios of the quantities of heat of each of these parts to the total quantity of heat of the spectrum under consideration, I obtained the results recorded in the following Tables.

TABLE I.—Impulsion Method.

Intervals.		Temperatures measured by the actinometer.			
		$t=10.$	$t=8.2.$	$t=7.2.$	$t=3.2.$
—1 24' to 0 16'	.....	0.088	0.042	0.031	0.0
0 16 „ 1 36	.....	0.443	0.441	0.433	0.307
1 36 „ 2 56	.....	0.404	0.419	0.435	0.528
2 56 „ 4 16	.....	0.064	0.098	0.101	0.164
		0.999	1.000	1.000	0.999

Platinum begins to melt when the actinometer gives 10.3.

Taking the ratios of the quantities of heat measured by the pile to those measured by the actinometer, we have for the temperatures 10, 8.2, 7.2, 3.2, the ratios 9.14, 5.86, 5.77, 4.38.

TABLE II.—Fixed-temperature Method.

Intervals.		Temperatures measured by the actinometer.				
		9.5.	7.4.	6.0.	4.6.	4.0.
—0 44' to 0 16'	.....	0.056	0.052	0.045	0.024	0.024
0 16 „ 1 36	.....	0.445	0.420	0.406	0.394	0.388
1 36 „ 2 56	.....	0.400	0.427	0.433	0.464	0.453
2 56 „ 4 16	.....	0.098	0.100	0.114	0.116	0.134
		0.999	0.999	0.998	0.998	0.999

If we take the ratios of the quantities of heat spread over the luminous part to the total quantities diffused throughout the spectrum, we have for the foregoing temperatures the numbers 0.030, 0.022, 0.012, 0.003, 0.

Taking, as before, the ratios of the quantities of heat indicated by the pile to those indicated by the actinometer, we have the numbers 7.09, 5.74, 5.53, 5.24, 5.02.

*Tables I. and II. show the variations of the distribution of heat in the spectrum with the temperature; and from them we infer that flint glass becomes less diathermanous as the temperature falls.*

Finally, by interposing in the path of the radiation from the source a solution of iodine in chloroform, I found that the minima, the existence of which we had previously recognized, all undergo parallel displacements proportional to the fall of the temperature. This assertion is verified by the following Table.

Temperatures.	Minima.		
	a.	b.	c.
9.8	1 20' to 1 24'	1 40' to 1 44'	2 0' to 2 4'
9.5	1 24' to 1 28	1 44' to 1 48	2 4' to 2 8
4.5	1 40' to 1 44	. . . . .	2 20' to 2 24
4.0	1 44' to 1 48	2 4' to 2 8	2 24' to 2 28
3.8	1 48' to 1 52	. . . . .	2 28' to 2 32

I also noticed that the above solution becomes more diathermanous when the temperature is lowered.

I am studying the spectra of other substances, and, in speaking of them, shall return to the last-mentioned subject.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 1153–1156.

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[FIFTH SERIES.]

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SEPTEMBER.

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XXII. *On Isomerism.* By M. M. PATTISON MUIR, *F.R.S.E.*,  
*Assistant-Lecturer on Chemistry, the Owens College, Man-*  
*chester* \*.

1. **I**N a paper published in this Magazine ([V.] vol. ii. p. 1) I upheld the proposition that our ordinary chemical notation indirectly expresses changes in the form of the energy of a system of bodies reacting chemically upon one another. Among the phenomena which are undoubtedly connected with such changes of energy isomerism holds an important position. I endeavoured to show that the facts of isomerism, while pointing to differences in the energies of the isomeric bodies, were not inconsistent with the generally accepted theory, according to which isomeric molecules are possessed of different atomic structures.

I shall now endeavour to supplement the remarks made in the paper referred to by others having a similar bearing.

2. The "position theory" of isomerism would seem to account for the phenomena by assigning to the atoms which form two isomeric molecules different positions in the two molecules; the "energy theory" of isomerism would trace the cause of the phenomena to those "conditions of action under which forces effect a fixed alteration in one or more functions of radicles."

But these theories are not, it seems to me, incompatible. Changes of function are associated with changes of energy, and these again with changes in the "configuration of a material system."

\* Communicated by the Author.

*Phil. Mag.* S. 5. Vol. 2. No. 10. Sept. 1876.

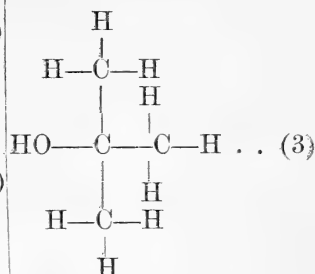
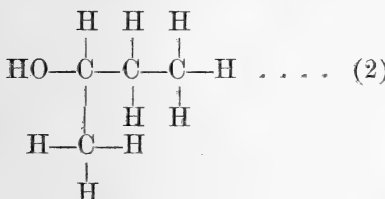
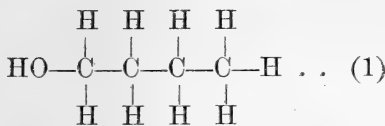
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Granted that there is an inner structure of the molecule, we should, *à priori*, expect that changes in this structure would be correlated with changes in the functions of the molecule. And we must imagine that atomic motions are continually taking place in the molecule; hence under certain conditions these motions may be directed so as to result in the development of a form of energy different from that which is developed when the atomic motions are otherwise directed. If it be urged that the terms "molecule," "atomic motion," &c. involve theoretical considerations, we grant the objection, if objection it be. The same objection would apply to very many of the most important dicta of science. Still it might be said that by limiting our attention to the changes of function (or of energy) we are dealing with facts, and with facts alone.

But if we speak of energy at all we must, I think, imagine a something which is "the seat or vehicle of energy;" to this something we give the name of matter. Hence a change of energy appears to involve in some way a change of matter. If the body, the energy of which changes form, is not possessed of a molecular and atomic structure, but is continuous, it must at any rate have a grained structure of some kind (otherwise many well-established phenomena remain utterly unaccounted for); and we can then imagine some kind of change in the position of these grains. Such a theory of infinite divisibility of matter does not, however, explain so many facts as the other (*i. e.* the molecular theory) does. We therefore prefer the molecular theory, and seek to associate with changes in the energy of chemical substances changes also in the structure of the matter composing these substances.

3. Of course it is very possible to put a wrong meaning upon the structural formulæ which are used to express the facts of isomerism. Three alcohols having the empirical formula  $C_4H_{10}O$  are known. These bodies do not undergo the same reactions with the same reagents. In the formation of these bodies "a fixed alteration has been produced in one or more of their functions." Would it then be enough to inquire "under what conditions of action" these alterations are produced? Surely not; we seek to determine whether there are differences in the inner mechanism of those small parts of the substances which remain united when the substances are hot. And when we have, as it would appear, learned that there are such differences, we of course seek to express these differences in the formulæ given to the substances. Thus to so-called normal butylic alcohol is generally assigned the structural formula  $CH_2(C_3H_7)OH$ ; to secondary butylic alcohol

the formula  $\text{CH}(\text{CH}_3)(\text{C}_2\text{H}_5)\text{OH}$ ; and to tertiary butylic alcohol the formula  $\text{C}(\text{CH}_3)_3\text{OH}$ . Graphically written these formulæ become



In many text-books these differences are expressed by some such statement as :—"in the primary alcohol the OH group is attached to a carbon atom which is itself attached to one other; in the secondary alcohol the OH group is attached to a carbon atom which is itself attached to two others; and in the tertiary alcohol the OH group is attached to a carbon atom which is itself attached to three other carbon atoms." Such expressions as "attached to a carbon atom at the beginning of the chain," "attached to a carbon atom in the middle of the chain," &c., are also used when speaking of isomerism. From these and similar expressions one might conclude, as some of the opponents of the position theory would almost appear to do, that the molecule is regarded as a fixed structure, the different parts of which are rigidly arranged around a centre (just as one might stick balls of clay on to stone), and that by removing one of the pieces which compose this molecule and placing it in another fixed position an isomeric molecule is produced.

Language which would give any countenance to such crude ideas as this should certainly not be made use of. The upholders of the position theory have given grounds for fault-finding. But is the whole theory to be rejected because of crudities in the manner of stating it?

That the structural and graphic formulæ quoted above do give to the chemist a very large amount of information concerning the three butylic alcohols is patent to all; and that the main idea implied in them, viz. that the atoms composing the molecules, although the same in kind and in number in

each of the three, are nevertheless not arranged in the same manner in each, is, I think, not opposed to but is in keeping with known facts.

4. Granting, then, that change in the form of energy and change of molecular structure are correlated in isomeric bodies, the inquiry into the facts of isomerism would naturally branch out into two directions.

Chemists would seek to determine what are the atomic arrangements in isomeric molecules, and what are the differences in the energies of these molecules.

The first part of this inquiry has been approached by a method which seems to present the most probable chances of success, viz. by studying the reactions of formation and of decomposition of isomers. These reactions have been in many instances generalized, and the results have been embodied in appropriate formulæ. From the generalizations so reached some slight idea of the atomic arrangement (granting, of course, that such arrangement has an actual existence) of many isomeric bodies has been attained to.

But even in this part of the inquiry we have still very much to learn. Attention has not, it appears to me, been sufficiently paid to the fact that atomic structure involves atomic motion of some kind.

In the second part of the inquiry (that, namely, which relates to changes of energy) little or no progress has been made. The subject is beset with many difficulties; but in this field many of the future triumphs of chemical science will doubtless be achieved.

5. As a preliminary investigation it would be well that the data concerning the physical properties of isomers should be more carefully observed. Something has, it is true, been done in this direction. Thus we know that the boiling-points of isomeric compounds, which have, so far as we can judge from their reactions, a similar molecular structure, are nearly identical; whereas when the molecular structure of isomeric compounds is essentially different we find a marked difference between the boiling-points of these compounds. We are indeed in some cases able to trace a somewhat regular alteration in the boiling-points of the members of an isomeric series and to correlate this with a regular alteration in molecular structure; thus the boiling-points of the four butylic alcohols differ by about  $11^{\circ}$ . By comparing the structural formulæ of these compounds (*i. e.* formulæ which generalize the reactions of formation and of decomposition which these bodies exhibit) a gradual closing-up, so to speak, of the molecular structure becomes apparent.



Normal butylic	}	$\text{CH}_3\text{—CH}_2\text{—CH}_2\text{—CH—OH}_2$	B.P = 116°.C.
alcohol . . .			
Isobutylic alcohol		$\text{CH}_3\text{—CH—CH}_2\text{—OH}$ $\text{CH}_3$	„ 109° „
Secondary butylic	}	$\text{CH}_3\text{—CH}_2\text{—CH—CH}_3$ $\text{OH}$ $\text{CH}_3$	„ 97° „
alcohol . . .			
Tertiary butylic alcohol		$\text{H}_3\text{C—C—CH}_3$ $\text{OH}$	„ 82° „

So, again, we learn from experiment that a substance whose reactions are best expressed by writing its formula as containing the group OH, is generally possessed of a higher boiling-point than another isomeric substance which does not appear to contain this group; hence from a consideration of the boiling-points of ethylic alcohol, and of methylic ether respectively (78° and −21° C.), and from a knowledge of the two possible modifications of the empirical formula ( $\text{C}_2\text{H}_6\text{O}$ ), which is common to each, we should expect, without a previous study of the reactions of these bodies, that the structural formula of the alcohol would be  $\text{CH}_3\text{—CH}_2\text{—OH}$ , and of the ether  $\text{CH}_3\text{—O—CH}_3$ .

More careful and extended observations of the boiling-points of isomeric compounds will doubtless throw considerable light on their molecular structure. The specific volumes of isomeric bodies are also, it would appear, connected with their molecular structure. Thus according to Kopp the specific volumes of acetic acid and methylic formate are almost identical; their structural formulæ are likewise analogous, viz.  $\text{CH}_3\text{—CO—OH}$  and  $\text{H—CO—O—CH}_3$  respectively.

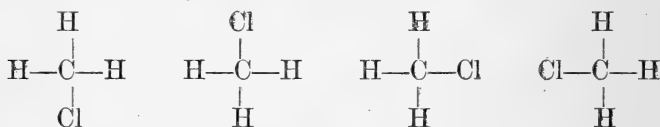
The researches of Thorpe, part of which have been already published, will probably help to elucidate the connexion which exists between specific volume and isomerism.

From a study of the heat of combustion and of formation of isomeric substances, we may also hope for knowledge concerning the second part of the general problem of isomerism. May we not suppose that in certain isomers the energy exists in a more available form than it does in others, perhaps even in other members of the same group? We know that under certain conditions energy may be in a more available form than under other conditions; is there such a difference in the energy of isomers? Supposing that this is the case, availability

will depend on the position of the parts; hence the two theories will be complementary of each other.

6. One of the difficulties in the way of explaining the facts of isomerism in terms of the molecular theory arises from our ignorance of the conditions which influence the linking-together of atoms in a molecule. Given the formula of a substance and the valency of the atoms, it is possible to arrange the symbols in a certain number of ways only; each of these methods of arrangement may represent an isomeric body. But we have often very good reasons for believing that of these possible methods of arrangement two or more will represent substances which are actually identical, not isomeric, with each other.

Thus if we replace one hydrogen atom in marsh-gas by chlorine we might arrange the symbols representing the resultant in four ways:—



Yet we know that but one substance is invariably produced in the foregoing reaction. Hence we conclude that the four "affinities" or four "equivalents" of carbon are, in this case, of equal value. In other instances, however, it would appear as if the "affinities" of an elementary atom were not all of equal value. Thus we know that phosphorus pentachloride,  $\text{PCl}_5$ , at a moderate temperature splits up into  $\text{PCl}_3$  and  $\text{Cl}_2$ ; hence we might suppose that two of the chlorine atoms are held to the phosphorus atom less firmly than the remaining three\*. Have we here the existence of weaker and stronger affinities in the phosphorus atom? If such differences actually occur, the existence of isomeric bodies may be traced to other causes than that which is generally understood by the phrase "position of the atoms."

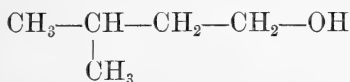
7. It would appear very probable that the valency of an atom may be regarded as an expression for the number of directions or of positions in which the atom is capable of exercising chemical force. Thus when we say that the carbon atom is tetravalent we mean, on this hypothesis, that it is capable of exercising chemical force in four directions, or that

\* It may be objected that we do not know that  $\text{PCl}_5$  actually exists. Wurtz's determinations of the vapour-density of this compound (*Compt. Rend.* vol. lxxvi. p. 601), carried out in an atmosphere of the trichloride whereby dissociation was prevented, show that  $\text{PCl}_3$  truly represents the molecular weight of the substance.

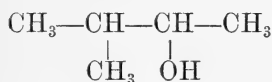
in each of its periodic movements there are four positions in which it can exercise such force. Now if this be granted we are in no way necessitated to suppose that the valency and the total chemical affinity are dependent on one another, nor that the force exercised in one of the four directions or positions is equal in amount to that exercised in each of the other three directions or positions. On this supposition the equivalents or affinities of an atom may differ in value; and the existence of isomers becomes possible, the difference between which is to be traced to the directions in which the chemical force of the constituent atoms of the molecule is exercised.

Such a theory as this has commended itself to several chemists, among others to Crum Brown, who has discussed some of its consequences in a paper published in the 'Journal of the Chemical Society' [2], vol. iii. p. 230.

But, on the other hand, it may be supposed that the equivalents of a given atom have equal values, and, nevertheless, that, owing to the positions of the atoms with respect to each other, isomeric bodies may arise which shall exhibit differences of behaviour towards the same reagents. Thus, of the isomeric amylic alcohols ( $C_5H_{11}OH$ ), that whose reactions are best expressed by the constitutional formula



is stable at temperatures at which that having the constitutional formula



undergoes dissociation into water and amylene.

The most plausible explanation of this fact appears to be, that, although the affinities of the carbon atoms are all of equal value, yet the group OH is related in one case to a carbon atom a large number of whose affinities are already "saturated" (to use a common term) with other carbon affinities; whereas in the other case the same group is related to a carbon atom whose affinities are not to so great an extent "saturated" with carbon. Hence, from the different positions occupied by the OH group, it is possible, during the excursions of the molecule, for those forces which tend to produce molecular disintegration more readily to act upon the OH group in the one case than in the other.

Again we may suppose that the affinities of any given atom are of equal value, but that as they are successively

saturated those which remain become weaker, if one may use such an expression, and that in some instances the atom does not exercise its total valency because of this weakening of the unsaturated affinities. On this view the whole of the atoms in a molecule must influence the total force exercised by that molecule. Thus the group OH may, in certain compounds, exchange its hydrogen for metals, while in others, or even in the same compounds, an exchange of hydrogen of this group can only be effected for alcoholic radicles.

The group has thus two functions, an "alcoholic" and an "acid" function. Now we cannot doubt that it is the composition of the remainder of the molecule which chiefly determines the function of the OH group; the position of the group will of course also exercise an influence. From these considerations it becomes evident that in considering the phenomena exhibited by a chemical molecule we may divide these phenomena into two classes:—first, those which are dependent on the nature of the atoms composing the molecule; and, secondly, those which are dependent on the linking-together or the position of these atoms in the molecule\*. The phenomena of isomerism will be influenced by both of these factors; and in endeavouring to explain these phenomena we must overlook neither.

8. We may, then, perhaps imagine the meeting of two molecules, the result of which is to be the formation of new molecules. Molecular dissociation is the first result; for a moment we may picture the atoms vibrating about certain fixed points, at each of which these atoms are capable of exercising their chemical affinity.

Let us suppose that the atoms of molecule *a* vibrate about four, and that the atoms of molecule *b* vibrate about two of these points. Let us further suppose that the force exercised at each of the four points by *a* atoms is different in amount, but that *b* atoms exercise the same amount of force at each of their two points.

The atoms become associated and new molecules result, which have a different atomic structure from that of the original molecules *a* and *b*, and are possessed of an amount of energy differing from that originally possessed by these molecules. But suppose the atoms of molecule *b* had come within the sphere of action of the atoms of molecule *a*, not at the two positions at which they have, by supposition, actually come within that sphere of action, but at the two remaining positions, we should have a second new set of molecules produced

\* See L. Meyer's *Moderne Theorien der Chemie*, 2nd ed. pp. 138–217; also Crum Brown, *loc. cit.*

composed of the same atoms as those which actually were produced, but differing in atomic structure, and in the amount, or at any rate in the form, of the energy possessed by them.

We have here isomerism which is chiefly due to a difference in the value of the equivalents of the atoms of one of the reacting molecules.

Let us now imagine that the atoms of the reacting molecules were capable, as before, of exercising chemical force in four and in two positions respectively, but that the force exercised in each position is the same in amount.

After the atomic interchanges have taken place (we may suppose, for a moment, that these interchanges are accomplished almost instantaneously, and that the resultant molecules are removed from the sphere of mutual action) we have new molecules, still, by supposition, capable of exercising chemical force in two directions; but we may surely suppose that the amount of force to be exercised in each of these directions is not the same *now* as it was before the atoms had become associated. The occupation, so to speak, of two of the positions of possible action will most probably influence the force which remains to be exercised at the two residual positions, even supposing that originally an equal amount of force was exercised at each of the four. The phenomena of magnetism and of electricity make us acquainted with actions somewhat similar to this.

If we may carry the imagination one step further and suppose that the occupation of two positions has not only influenced the force to be exercised at the remaining positions, but has also influenced it unequally, we shall now have a molecule which is capable of forming isomers by a process similar to that which was carried out in the case which we first attempted to follow.

If we grant that the occupation of a certain number of the positions at which an atom is capable of exercising chemical force, by another atom, causes a change in the amount of force remaining to be exercised at each of the residual positions, we may also very well grant that upon the nature of the occupying atom will greatly depend the extent of this influence.

By substituting a new occupying atom of equal valency with the original we shall produce not only a new molecule, but a new molecule which is possessed of a different amount of energy from that originally formed. On this supposition we can trace the formation of isomers containing different atomic groups.

Let us suppose that two molecules *a* and *b* come within the

sphere of one another's action; that molecular dissociation occurs, but that in the case of molecule *b* this is not carried out completely, that instead of the setting-free of single atoms, groups (or at any rate *a* group) of atoms are produced; these groups become combined with the atoms which formerly constituted molecule *a*: of the equivalents of these atoms some are satisfied by the new atomic groups; and in this process the remaining equivalents become altered in value (we have supposed that originally the equivalents were all of equal value). Now, on our supposition, the nature of the atomic group or groups which occupy certain of the positions in which the atoms of molecule *a* are capable of exercising force, will influence the amount of force remaining to be exercised at the unoccupied positions; hence we have only to imagine for the production of an isomer that molecule *a* comes within the sphere of action of a third molecule *c* composed of the same atoms as *b*, but capable of splitting up into different atomic groups.

In these supposed formations of isomers we have change of position, change of valency, and change of energy taking place simultaneously.

9. So-called physically isomeric bodies\* appear rather to belong to the category of polymeric substances. In most recorded cases it is very probable that the molecular weight of the substance does not remain constant throughout its modifications. If this be so, we should of course have no difficulty in explaining why varying phenomena are presented by each of the modifications.

Laubenheimer (*loc. cit.*) seeks to explain the formation of so-called physically isomeric bodies by the supposition that the molecules of those modifications which have the higher melting-points are made up, so to speak, of a greater number of fundamental molecules ("*Grundmoleküle*") than the molecules of modifications which melt at lower temperatures. This supposition takes for granted that molecular compounds exist, or at any rate that by the aggregation of molecules (not of atoms) new molecules may be produced,—molecule being taken to mean the smallest portion of a body which holds together when the body is hot.

In a paper published elsewhere† I used the expression "molecular valency" as being perhaps applicable to certain

\* Duffy, Chem. Soc. Journ. vol. v. p. 197; Zincke, *Deut. Chem. Ges. Ber.* vol. iv. p. 576; Tollens, *ibid.* vol. viii. p. 1427; Laubenheimer, *ibid.* vol. ix. p. 760, &c. &c.

† "On Recent Chemical Researches," Quart. Journ. of Science, January 1876.

cases of formation of so-called molecular compounds. The use of this term is based on the assumption, which appears to be in keeping with facts, that, although in a given molecule "the lines of force of its atoms are mutually satisfied, there may be directions in which the molecule, as such, may exert a certain amount of chemical energy."

This idea follows naturally from the view of valency and chemical affinity which I have sketched in paragraph 7 of the present paper, because, if the valency of an atom be an expression for the number of directions or positions in which that atom is capable of exercising force, but if the amount of force exercised be independent of the valency, it follows that, although an atom exercise its force in each of the possible directions, a part of that force may possibly yet remain unexercised. And may we not imagine that this unexercised force may be exercised by the newly formed molecule, and that in one or more directions?

A view somewhat similar to this was put forward by Mills some years ago\*. If we grant "molecular valency," we can easily see that the formation of physically isomeric bodies becomes possible according to the supposition that the difference between these bodies is to be traced to the placing together of a greater or less number of "fundamental molecules" in the molecule of each.

But if compounds composed of the same number of the same atoms do exist the only difference in whose properties is a purely physical one, we may still, I think, be able to explain the existence of such truly "physically isomeric" bodies, either upon the supposition that the affinities of many atoms differ in value, or by supposing that these affinities are originally of equal value, but that the "saturation" of some of them modifies those which remain.

10. It has been supposed that those elements which are capable of assuming allotropic forms may sometimes exist in these forms in their compounds†. The existence of allotropic forms of elementary substances is closely allied with the existence of isomeric forms of compound substances. The same or very similar explanations are probably applicable in both cases; in the case of elements we shall have to deal with atomic interchanges of similar atoms only. If we grant variability of affinities, we may suppose that the nature of the particular allotrope produced by the mutual action of two ele-

\* Phil. Mag. [IV.] vol. xxviii. p. 364.

† See Weber in this Magazine [IV.] vol. xlix. p. 300; also Wiedemann, Pogg. Ann. vol. clvii. p. 41, a translation of which appears in the August Number of the Philosophical Magazine.

mentary atoms will be influenced by the position of the affinities mutually satisfied; or if we prefer to regard the elementary atomic affinities as of equal value, we may still suppose that the mutual action of two elementary atoms will so modify the residuary affinities as to render possible the formation of allotropic forms according to the position in the molecule occupied by a third atom, which now comes within the sphere of action of the molecule. Or it may very possibly be that both of these causes influence the formation of allotropes. We certainly know that the density of some allotropic forms of the same element is not a constant number; it appears also that changes of energy accompany the changes of an elementary molecule from one allotropic form to another.

11. Isomerism does not appear to occur among inorganic compounds to so marked an extent as among the compounds of carbon. A variety of silver chloride has been described by Field\*, which is unacted upon by light, and is distinctly crystalline; so, also, we know of two modifications of nitrogen tetroxide,  $N_2O_4$  and  $NO_2$ . A systematic search for isomeric bodies among the non-carbon compounds would doubtless meet with a considerable degree of success.

XXIII. *Supplementary Discussion of the Hydrodynamical Theory of Attractive and Repulsive Forces.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.†

1. **I**N the mathematical theories of the physical forces which from time to time I have proposed and discussed in this Journal, the reasoning has been uniformly made to rest on the following principles:—

All visible and tangible substances consist of atoms.

An atom is a very small inert spherical mass of constant form and magnitude.

All space not occupied by atoms is filled by a perfectly fluid substance (the æther), of uniform pressure and density when at rest, and varying in pressure when in motion in exact proportion to variations of its density.

All physical force acts either as pressure of the æther upon the surfaces of atoms, or as reaction of the atoms upon the æther by reason of the constancy of their form and magnitude.

2. The selection of these hypotheses has been determined

\* Chem. Soc. Journ. vol. x. p. 242.

† Communicated by the Author.



by two regulative principles :—(1) they must be perfectly intelligible from sensation and experience ; (2) they are required to be appropriate foundations of mathematical reasoning. The æther, being assumed to be susceptible of variation of density, must be conceived to be atomically constituted, because we have no experience of variation of density and pressure which is not the result of atomic constitution. But for the purposes of physical research, it suffices to regard the æther as a *continuous* substance, and to apply calculation to it as such, just as the air is treated mathematically in Hydrodynamics, although it is known to be composed of discrete atoms. After adopting these views, the atoms of the æther do not *individually* come under consideration.

3. In this philosophy the only admissible form of force is *pressure*, because we have distinct perception of such force, and such only, by personal *act* and *sensation*, as when the hand is pressed against any substance.

4. According to the foregoing principles of theoretical physics it is not allowable to make any assumption respecting the laws of action of the physical forces, inasmuch as these laws have to be deduced by strict mathematical reasoning from the above stated hypotheses. This I consider to be the legitimate application of the rules and principles of philosophy suggested by Newton at the beginning and the end of the third book of the 'Principia.' To deduce exclusively from these hypotheses the modes of operation of the various physical forces, and to compare the theoretical results with facts of observation and experiment, might well seem to be an arduous task. Since, however, the Newtonian principles of philosophy can only by such means be tested or established, an upholder of those views could not do otherwise than enter upon this undertaking. Accordingly I have devoted much time and research to problems the solutions of which are demanded by this course of philosophy ; and, in particular, I have endeavoured to infer the laws of attractive and repulsive forces from modes of pressure of the ætherial medium. The furthest advance I have made towards the solution of this essentially important but difficult problem is given in arts. 30–39 of a communication contained in the Number of the Philosophical Magazine for September 1872. The object of the present communication is to correct in certain respects that investigation and to carry it a step further ; and with this object in view I shall begin with reproducing here so much of the antecedent argument as may be required for making intelligible the proposed extension of it.

5. In the Philosophical Magazine for August 1862, but

more accurately and completely in pages 201–228 of ‘The Principles of Mathematics and Physics,’ I have obtained particular solutions of the general hydrodynamical equations applicable to the motion of a fluid for which  $p = a^2 \rho$ , without having made previously any supposition as to the circumstances under which the fluid was put in motion. These results, as depending on no special arbitrary conditions, but only on the general analytical hypothesis that  $u dx + v dy + w dz$  is an exact differential, are considered to give the laws of *spontaneous* mutual action between the parts of the fluid. The motion thence resulting is characterized by being symmetrical with respect to an *axis*, and by consisting wholly of harmonic vibrations partly parallel and partly transverse to the axis.

6. It is not necessary to introduce here the details of the abstract reasoning whereby this particular motion was reached; but it is important to remark that as the solution is unique and definite it cannot without error be left out of consideration in applications of *general* solutions of the hydrodynamical equations, and that it must be taken into account *before* proceeding to apply such solutions to cases of motion taking place under given arbitrary circumstances. This being premised, I shall now adduce the investigation of that particular motion in sufficient detail for the present purpose; and in order to give the means of acquiring fuller information, references will be made to the pages of the above mentioned work, or to Numbers of the Philosophical Magazine, in which the several steps of the reasoning are completely discussed.

7. A differential equation, which I call the first general equation of hydrodynamics, being formed on the principle that the directions of motion in each *given* element are normal to surfaces of displacement in *successive instants*, it may be shown that if  $u dx + v dy + w dz$  be an exact differential ( $d\psi$ ), the motion is spontaneously rectilinear where  $\psi$  has a maximum or minimum value (Props. VII. and XI. in pages 186 and 201; the reasoning is given better in arts. 32–35 of the communication in the Philosophical Magazine for September 1872). Accordingly, assuming that there would be rectilinear motion along an *axis*, it may be supposed, for the rest of the motion, that  $(d\psi) = (d.f\phi)$ , and, the ordinates  $z$  being reckoned along that axis, that  $f$  is a function of  $x$  and  $y$  only, and  $\phi$  a function of  $z$  and  $t$  only. These assumptions are justified by actually finding definite expressions for the functions  $\phi$  and  $f$  by purely analytical reasoning. It is found, in fact, by such process (pages 201–205 and 210) that, to the first approximation,

$$\phi = -\frac{m\lambda}{2\pi} \cos \frac{2\pi}{\lambda} \left\{ z - at \left( 1 + \frac{e\lambda^2}{\pi^2} \right)^{\frac{1}{2}} + c \right\},$$

$$f = 1 - er^2 + \frac{e^2 r^4}{1^2 \cdot 2^2} - \frac{e^3 r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c.$$

In these expressions  $e$  is a constant introduced by fulfilling the condition that  $\phi$  is a function of  $z$  and  $t$  and  $f$  a function of  $x$  and  $y$ ;  $c$  is a constant altogether arbitrary;  $m$  determines the intensity and  $\lambda$  the extent of the vibrations; and  $r$  is the distance of any point from the axis. It is proved also (Prop. XIV. pages 214–224) that the factor  $\left( 1 + \frac{e\lambda^2}{\pi^2} \right)^{\frac{1}{2}}$  has the fixed numerical value 1.2106. The exact determination for determining  $\phi$  is found to be (p. 203)

$$4a^2 e \phi - a^2 \frac{d^2 \phi}{dz^2} + \frac{d^2 \phi}{dt^2} + 2 \frac{d\phi}{dz} \cdot \frac{d^2 \phi}{dz dt} + \frac{d\phi^2}{dz^2} \frac{d^2 \phi}{dz^2} = 0. \quad (\alpha)$$

If, for the sake of shortness,  $\kappa$  be put for  $\left( 1 + \frac{e\lambda^2}{\pi^2} \right)^{\frac{1}{2}}$ ,  $\zeta$  for  $z - \kappa at + c$ , and  $q$  for  $\frac{2\pi}{\lambda}$ , this equation gives by successive approximate integrations (p. 206),

$$\frac{d\phi}{dz} = m \sin q\zeta - \frac{Am^2}{a} \cos 2q\zeta - \frac{Bm^3}{a^2} \sin 3q\zeta + \&c.,$$

A being put for  $\frac{2\kappa}{3(\kappa^2 - 1)}$ , and B for  $\frac{3(7\kappa^2 + 1)}{32(\kappa^2 - 1)^2}$ .

After making the more general assumption (pp. 226 & 227) that

$$\psi = -\frac{mf}{q} \cos q\zeta - \frac{m^2 Ag}{2qa} \sin 2q\zeta + \frac{m^3 Bh}{3qa^2} \cos 3q\zeta,$$

I found, on substituting this value in the general differential equation of which  $\psi$  is the principal variable, that the factors  $g$  and  $h$ , as well as  $f$ , are determinable as functions of  $r$ , the results being  $f = 1 - er^2 + \&c.$  (as before),

$$g = 1 - er^2 - \frac{3\kappa^2 - 1}{4} e^2 r^4 + \&c., \quad h = 1 - er^2 + \&c.$$

Then, as the differential coefficients  $\frac{d\psi}{dz}$  and  $\frac{d\psi}{dr}$  give for any point the velocities parallel and transverse to the axis, the condensation at the same point is obtainable from the equation

$$a^2 \text{Nap. log } \rho + \frac{d\psi}{dt} + \frac{1}{2} \left( \frac{d\psi^2}{dz^2} + \frac{d\psi^2}{dr^2} \right) = 0. \quad (\beta)$$

Thus the circumstances of spontaneous *uncompounded* motion relative to an axis are in all respects determined, and with any required degree of approximation.

8. This result is of primary importance relatively to the mathematical theory of *musical vibrations*, as will appear from the following considerations. In the case of *aerial vibrations*, for which within the usual compass of musical notes  $\lambda$  is large and consequently, as  $e\lambda^2$  is constant,  $e$  is very small, each of the factors  $f, g, h$ , &c. will be nearly unity, and we may suppose  $\frac{d\Psi}{dz}$  to vanish. Then for the velocity in the direction of  $z$  we have

$$\frac{d\Psi}{dz} = m \sin q\zeta - \frac{m^2 A}{a} \cos 2q\zeta - \frac{m^3 B}{a^2} \sin 3q\zeta + \&c.$$

Now as this expression has been obtained prior to any supposition as to the mode of disturbing the air, it is to be taken into account in any case of motion due to *arbitrary* disturbance. Representing therefore by  $\Psi$  the value of the principal variable when it applies to motion produced under given arbitrary circumstances, let us suppose, in order to fulfil the above-mentioned condition, that

$$\begin{aligned} \frac{d\Psi}{dz} = & \Sigma_1 \cdot \left[ m \sin q_1 \zeta - \frac{Am^2}{a} \cos 2q_1 \zeta - \frac{Bm^3}{a^2} \sin 3q_1 \zeta + \&c. \right] \\ & + \Sigma_2 \cdot \left[ m' \sin q_2 \zeta' - \frac{Am'^2}{a} \cos 2q_2 \zeta' - \frac{Bm'^3}{a^2} \sin 3q_2 \zeta' + \&c. \right] \\ & + \&c., \end{aligned}$$

the symbol  $\Sigma$  signifying the sum of any number of such expressions as that contained within the brackets. For these expressions  $m$  may be supposed to have the respective arbitrary values  $m_1, m_2, m_3$ , &c., and  $c$  in  $\zeta$  to have the corresponding

arbitrary values  $c_1, c_2, c_3$ , &c., whilst  $q_1$ , or  $\frac{2\pi}{\lambda_1}$ , has the same

value for all the expressions embraced by the first sum ( $\Sigma_1$ ). Similarly for those embraced by the second sum ( $\Sigma_2$ ), the arbitrary quantities are  $m'_1, m'_2, m'_3$ , &c., and  $c'_1, c'_2, c'_3$ , and  $q_2$ , or  $\frac{2\pi}{\lambda_2}$ , is the same in all. If now account be taken of the arbitrary

quantities that are thus at disposal, it may readily be shown that the above equation may be put under the form

$$\begin{aligned} \frac{d\Psi}{dz} = & C_1 \sin(q_1 z + \alpha_1) + C_2 \sin(2q_1 z + \alpha_2) + C_3 \sin(3q_1 z + \alpha_3) + \&c., \\ & + C'_1 \sin(q_2 z + \alpha'_1) + C'_2 \sin(2q_2 z + \alpha'_2) + C'_3 \sin(3q_2 z + \alpha'_3) + \&c., \\ & + C''_1 \sin(q_3 z + \alpha''_1) + \&c., \end{aligned}$$

in which equation, it is to be observed, all the terms are periodic in such manner that the mean of all the values of  $\frac{d\Psi}{dz}$  at any given instant is zero. Since the constants  $C_1, C_2, C_3, \&c.$ ,  $\alpha_1, \alpha_2, \alpha_3, \&c.$ , are arbitrary quantities, and not limited in number, we may by the application of *Fourier's Theorem* draw the inference that the series of terms in the first line of the above expression for  $\frac{d\Psi}{dz}$  is capable of representing *any* arbitrary form of vibration satisfying the condition that at a given instant the velocities at positions separated by the constant interval  $\lambda_1$  are equal. From this conclusion it follows that the production in any manner of a musical note whose wave-length is  $\lambda_1$  involves the simultaneous production of musical notes whose wave-lengths are  $\frac{\lambda_1}{2}, \frac{\lambda_1}{3}, \&c.$  Experience confirms this result, the ear being able to detect, together with the *fundamental* note corresponding to the wave-length  $\lambda_1$ , *harmonic* notes, as they are called, corresponding to the other wave-lengths. If we take the first and second series of terms in the expression for  $\frac{d\Psi}{dz}$ , and suppose the ratio of  $\lambda_1$  to  $\lambda_2$  to be exactly equal to that of two low numbers, we shall have the theoretical expression of musical *concord*s. If  $\lambda_1$  be to  $\lambda_2$  in a ratio expressed by that of two high numbers nearly equal, the same two series will express the phenomenon of *beats*. In short, by taking *ad libitum* the number of the series and corresponding wave-lengths, we can express every species of *sound*, from a pure musical note accompanied by its harmonics to irregular and confused *noise*; and it may, I think, be regarded as evidence that sounds have actually the composite character indicated by this theory, that noises partake so far of the qualities of notes as to be recognizable by the ear as being in some cases grave and in others acute.

9. It will now be proper to introduce certain considerations which will point out the bearing of the foregoing argument on the subject signified by the title of this communication. It has been shown how it is possible, by the aid of Fourier's theorem, to give a complete mathematical theory of aerial sounds, whether musical or unmusical. But this theory alto-

gether depends on the *à priori* determination of the laws of those aerial vibrations which I have designated as *spontaneous*, which, apart from any particular mode of disturbing the air, are found to be expressible by definite mathematical formulæ. The reality of this class of vibrations had been established by experience, inasmuch as it had been ascertained that independently of particular producing circumstances sounds are recognized by the sense of hearing as consisting of *fundamental notes* and accompanying *harmonics*. Obviously, therefore, the accounting for such vibrations by abstract argument founded on the general equations of hydrodynamics was a problem demanding solution. The appropriate solution I claim to have given by the process the steps of which are indicated in arts. 5–7 of the present communication. In this research no use was made of Fourier's theorem, because that theorem depends on principles that are analytical, and only as such admits of being applied in physical questions. But after the laws of the primary aerial vibrations have been deduced independently from hydrodynamical principles, Fourier's theorem is properly applied in proving that all sounds may be analytically represented by sums of such vibrations, and consequently in justifying the expression for  $\frac{d\Psi}{dz}$  assumed in

art. 8. For these reasons I maintain that the mathematical investigation of spontaneous vibrations, which I propounded originally in the Number of the Philosophical Magazine for February 1849, is an indispensable part of the theory of *Acoustics*, being required to fill up a *lacuna* whereby the *logic* of that theory would otherwise be vitiated. I have dwelt the more on the foregoing evidence of the reality of spontaneous vibrations of the air, because the same class of vibrations in the æther will have to be taken into account in the theory I now proceed to give of attractive and repulsive forces due to ætherial undulations. Other physical forces are more immediately referable to steady motions than to vibrations of the æther.

10. The æther being by hypothesis a perfect continuous fluid defined by the relation  $p = a^2\rho$  between its pressure  $p$  and density  $\rho$ , I propose, in the first instance, to attempt the solution of the following definite problem:—A series of plane periodic waves of the æther being incident on a very small fixed sphere of given radius, it is required to find the pressure at any point of the surface of the sphere at any time.

It is supposed that in plane periodic waves the motion is in directions perpendicular to a fixed plane and wholly vibratory, and that the velocity is at each instant a function of the dis-

tance from the plane. As preliminary to the solution of our problem, we have to determine under what hydrodynamical conditions such motion takes place. As long ago as the year 1848 I pointed out that the exact solution, according to the usually received principles of hydrodynamics, of the problem of plane-waves conducted to an absurd result, indicative of defect of principle. Subsequently I ascertained that the failure of the reasoning arose from not previously taking into account the primary spontaneous vibrations, which, as already argued in art. 8, it is logically necessary to employ for satisfying the conditions of arbitrarily imposed motion. In the theory of *sounds* it appeared that the required conditions could be fulfilled by means of *sums* of spontaneous vibrations (arts. 8 and 9). In like manner, as I am about to show, the conditions of the problem of plane-waves may be fulfilled by sums of uncompounded ætherial vibrations. It should, however, be noticed that whereas for aerial vibrations the transverse motions might be neglected and the factors  $f, g, h$ , &c. each be taken to be unity, this is no longer the case when we have to deal with such small wave-lengths as those which pertain to vibrations of the æther. Considering, however, that in the proposed application of the formulæ for the primary vibrations the distance  $r$  from the axis will always be very small compared with  $\lambda$ , and that the constant  $e$  varies inversely as  $\lambda^2$ , it will suffice to take  $f$  and  $g$  each equal to  $1 - er^2$ . Also, on account of the smallness of the factor  $\frac{m^2}{a^2}$  for the æther, it will not be necessary to include in the expression for  $\psi$  (art. 7), the terms beyond that which contains  $m^2$ . These limitations being admitted, we may proceed to find approximate expressions, in the case of a set of uncompounded spontaneous vibrations, for the direct and transverse velocities and for the condensation at any point situated at a given distance from the axis.

11. Accordingly, reverting to the expression of  $\psi$  in art. 7, we shall now suppose that

$$\psi = -\frac{mf}{q} \cos q\zeta - \frac{m^2 A f}{2qa} \sin 2q\zeta,$$

$f$  being equal to  $1 - er^2$ . Also since the numerical value of  $\kappa$  given in art. 7 was derived from the equation  $\kappa^4 = \frac{1}{\kappa^2 - 1}$ , and

$A$  was substituted for  $\frac{2\kappa}{3(\kappa^2 - 1)}$ , it follows that  $A = \frac{2\kappa^5}{3}$ . Hence,

putting  $\sigma$  for  $\rho - 1$ ,  $w$  for  $\frac{d\psi}{dz}$ ,  $\omega$  for  $\frac{d\psi}{dr}$ , and deducing from

the equation ( $\beta$ ) in art. 7 the value of  $\sigma$  to terms of the second order with respect to  $m$ , the following results may be obtained :—

$$w = f \left( m \sin q\zeta - \frac{2\kappa^5 m^2}{3a} \cos 2q\zeta \right);$$

$$\omega = -\frac{df}{dr} \left( \frac{m}{q} \cos q\zeta + \frac{\kappa^5 m^2}{3qa} \sin 2q\zeta \right);$$

$$\sigma = \frac{\kappa w}{a} + \frac{m^2}{2\kappa^4 a^2} \left( f^2 \sin^2 q\zeta - \frac{1}{4e} \frac{df^2}{dr^2} \cos^2 q\zeta \right).$$

Now the principle already adopted in the foregoing theory of musical sounds requires that the plane-waves of the present problem should be supposed to be made up of *sums* of the spontaneous vibrations defined by the above three equations. I have been compelled, by a *logical* necessity, to abandon the hypothesis of uncompounded plane-waves such as are usually admitted in hydrodynamics, having obtained absurd results when the reasoning on that hypothesis was made to include terms of the second order. It will therefore be supposed that the plane-waves of our problem are composed of an unlimited number of spontaneous vibrations, all in the same phase of vibration and having their axes all parallel, it being presumable that under these conditions the transverse vibrations will be wholly neutralized.

12. The diameter of the sphere being assumed to be excessively small compared with the wave-length  $\lambda$ , the condensation of the incident waves at any point of the sphere's surface will be very nearly the same for the composite waves above described as if they were uncompounded plane-waves. In fact, as far as regards the hemispherical surface on which the waves are immediately incident, the two cases may be treated as identical; but the amount of condensation at points of the other hemispherical surface will, in the case of the composite waves, depend in some measure on *limited* lateral divergence due to the composition, as will be presently indicated. This effect is smaller as the size of the sphere is less; so that for a first approximation we may assume that the condensation on both halves of the surface of a sphere of the size of an *atom* is the same as if the incident plane-waves were not composite. Since the motions in these waves consist of *harmonic* vibrations which produce no residual motion of translation of any particle of the æther, for given increments ( $dz$ ) of space the increments of condensation in the condensed portion of a wave must at each instant be just equal to the decrements; and this must be the case also in the rarefied portion, although



the moving force of the æther is greater in the condensed half of the wave than in the rarefied half in the proportion of the greater density.

13. It is evident that the condensation of an *uncompounded* wave at any point of a plane cutting transversely a diameter of the sphere drawn in the direction of the incidence of the waves will be the same at all points of this plane, excepting so far as the reaction of the sphere produces variation of the condensation at and near the spherical surface. The condensation immediately due to this reaction will be extremely small, because it contains as a factor the ratio of the diameter of the sphere to the wave-length ('Principles,' p. 283). Hence the accelerative action on the sphere will be very nearly proportional to the moving force of the fluid in a transverse plane passing through the centre of the sphere (see art. 33 of the communication on Attractive and Repulsive Forces, in the Philosophical Magazine for September 1872). Consequently, by the argument at the end of art. 12, there will be no tendency in the incident waves to produce any residual motion of translation of the sphere. Also, if we take account of the condensation to which the variation of the motion of the fluid along the surface of the sphere is due, no translatory action will result therefrom, because this motion is just as much accelerated on one hemispherical surface as it is retarded on the other, the velocity being zero at each extremity of the before-mentioned diameter, and the condensation being symmetrical with respect to a transverse plane passing through the centre of the sphere. This, however, does not apply to condensation due to *composite* plane-waves, because by reason of the composition the condensation on the *second* hemispherical surface undergoes alteration, the mode and amount of which it is the purpose of the following investigation to determine.

14. It is, first, to be remarked that those component vibrations the directions of the axes of which cut the surface of the sphere will on that account be *intercepted*. But notwithstanding this circumstance, inasmuch as the incident component waves, so long as they are not interrupted, comport themselves as if they were not compounded, the amount of condensation at any point of the *first* hemispherical surface will in either case be the same. This identity, however, does not hold good as respects the other hemispherical surface. For, conceive the sphere to be cut by any plane transverse to the direction of incidence, and to be enveloped by a cylindrical surface the axis of which is in the same direction; then the component vibrations whose axes pass through the portion of this plane which lies between the sphere and the cylindrical surface, will

contribute nothing to the condensation at the surface of the sphere if the plane cuts the *second* hemispherical surface, because in that case their axes are intercepted by the first hemispherical surface; whereas the corresponding components will be fully effective, as respects their condensation, at the *first* half-surface, because, although they are *reflected* at this surface, the effect of the condensation accompanying this reflection may, by what is said in art. 13, be left out of consideration. It appears from this argument that to find the whole accelerative action on the sphere we have only to find the accelerative action, on the first hemispherical surface, of so much of the incident waves as is composed of the *intercepted* vibrations, inasmuch as this action is not opposed by corresponding condensations on the second hemispherical surface.

15. The investigation will be sufficiently approximate for our purpose after making the following simplifications of the expressions for  $w$ ,  $\omega$ , and  $\sigma$  in art 11. Since  $\kappa^2 = 1 + \frac{e\lambda^2}{\pi^2}$ , and  $\kappa^2 - 1 = \frac{1}{\kappa^4}$ , it follows that  $e = \frac{\pi^2}{\kappa^4\lambda^2}$ , and that the value of  $f$  (art. 10) becomes  $1 - \frac{\pi^2 r^2}{\kappa^4\lambda^2}$ . As, for each set of component vibrations, we shall have to take account only of distances  $r$  from its axis which are less than the diameter of the sphere, and the ratio of this diameter to  $\lambda$  is by hypothesis very small, it will be seen that the second term of the expression for  $f$  will always be excessively small compared with the first, and in the present investigation may be neglected. Thus we have  $f=1$ ,  $\frac{df}{dr} = 0$ , and consequently

$$w = m \sin q\zeta - \frac{2\kappa^5 m^2}{3a} \cos 2q\zeta,$$

$$\omega = 0,$$

$$\sigma = \frac{\kappa w}{a} + \frac{m^2}{2\kappa^4 a^2} \sin^2 q\zeta$$

$$= \frac{\kappa m}{a} \sin q\zeta - \frac{m^2}{a^2} \left( \frac{2\kappa^6}{3} + \frac{1}{4\kappa^4} \right) \cos 2q\zeta + \frac{m^2}{4\kappa^4 a^2},$$

the value of  $w$  being substituted in the expression for  $\sigma$ . Accordingly these values of  $w$ ,  $\omega$ , and  $\sigma$  relative to a *given* axis apply to *all* points of the portion of the transverse plane contained between the spherical and cylindrical surfaces. Taking C to be the position of the centre of the sphere and P a point on its surface, let the line PC make an angle  $\theta$  with the line CO drawn from C to the origin O of the ordinates  $z$  in the di-

rection *opposite* to that of the incidence and propagation of the waves. Then, if  $b$  be the radius of the sphere, the above-mentioned portion of the transverse plane is equal to  $\pi b^2 - \pi b^2 \sin^2 \theta$ , or  $\pi b^2 \cos^2 \theta$ . Also, if PM be perpendicular to the axis of the motion, and if OM =  $z$  and OC =  $h$ , we have  $z = h - b \cos \theta$ , and  $z - \kappa at + c = h + c - b \cos \theta - \kappa at$ . In the subsequent reasoning only the first power of the small ratio  $\frac{b \cos \theta}{\lambda}$  will be taken into account, and  $c_0$  will be substituted for  $h + c$ .

16. Assuming now that in the incident waves the number of axes which pass through a unit of space is  $kn$ ,  $n$  being a certain *standard* number and  $k$  an *arbitrary* factor, the number which will pass through the portion  $\pi b^2 \cos^2 \theta$  of the transverse plane is  $kn\pi b^2 \cos^2 \theta$ . Hence, if  $w$  and  $S$  represent respectively the composite velocity and condensation under the supposed conditions of the problem, we have (by the principle stated in art. 10)  $W = knw$  and  $S = kn\sigma$ . Hence, supposing that

$$T = m \sin q(c_0 - \kappa at) - \frac{\kappa^5 m^2}{3a} \cos 2q(c_0 - \kappa at),$$

$$T' = \frac{\kappa m}{a} \sin q(c_0 - \kappa at) - \frac{m^2}{a^2} \left( \frac{2\kappa^6}{3} + \frac{1}{4\kappa^4} \right) \cos 2q(c_0 - \kappa at),$$

we shall have, to terms inclusive of the first power of  $\frac{b \cos \lambda}{\lambda}$ ,

$$W = \pi knb^2 \cos^2 \theta \left( T + \frac{dT}{\kappa a dt} \cdot b \cos \theta \right),$$

$$S = \pi knb^2 \cos^2 \theta \left( T' + \frac{dT'}{\kappa a dt} b \cos \theta + \frac{m^2}{4\kappa^4 a^2} \right).$$

Considering, first, the expression for  $S$ , we may omit the terms containing  $T'$  and  $\frac{dT'}{dt}$  as factors, because they are indicative of pressures which are wholly periodic at every point of the sphere's surface, and consequently give rise to no permanent accelerative action. The other part of the value of  $S$  is independent of  $t$ , and shows that the sphere is continuously pressed in the direction of the incidence of the waves. The pressure at any point P being  $a^2 S$ , the pressure on the whole hemispherical surface estimated in the direction of incidence is

$$\frac{\pi knb^2 m^2}{4\kappa^2} \int 2\pi b^2 \sin \theta \cos^3 \theta d\theta,$$

taken from  $\theta = 0$  to  $\theta = \frac{\pi}{2}$ . This gives for the total moving force

$$\frac{\pi^2 knb^4 m^2}{8\kappa^4}.$$

17. We have next to take account of the effect of the velocity  $W$ . Resolving it into parts  $W \sin \theta$  and  $W \cos \theta$  respectively along and perpendicular to the surface of the sphere, the latter may, for reasons already given, be left out of account, so that we have only to calculate the accelerative action on the sphere resulting from the velocity  $W \sin \theta$ . This velocity takes place along the intersection of the spherical surface by any plane passing through the axis from which  $\theta$  is measured, inasmuch as the motion is evidently symmetrical with respect to this axis. Thus we have here a case of *constrained* motion uninfluenced by the action upon the fluid of any extraneous accelerative force. Hence, if  $V$  be the velocity at any point at any time  $t$ , and  $\sigma'$  be the corresponding condensation, we shall have by a known formula,

$$\frac{\kappa^2 a^2 d\sigma'}{(1 + \sigma') b d\theta} + \left( \frac{dV}{dt} \right) = 0,$$

or

$$\frac{d\sigma'}{1 + \sigma'} = - \frac{b}{\kappa^2 a^2} \frac{dV}{dt} d\theta - \frac{V dV}{\kappa^2 a^2}.$$

But since  $V = W \sin \theta$ , we get by means of the value of  $W$  in art. 16,

$$\frac{dV}{dt} = \pi k n b^2 \sin \theta \cos^2 \theta \left( \frac{dT}{dt} + \frac{d^2 T}{\kappa a dt^2} b \cos \theta \right).$$

Hence, by substituting in the last equation and integrating,

$$\begin{aligned} \text{Nap. log } (1 + \sigma') &= F(t) \\ &+ \frac{\pi k n b^3}{\kappa^2 a^2} \left( \frac{dT}{dt} \frac{\cos^3 \theta}{3} + \frac{b}{\kappa a} \frac{d^2 T}{dt^2} \cdot \frac{\cos^4 \theta}{4} \right) - \frac{V^2}{2 \kappa^2 a^2}. \end{aligned}$$

Since by the conditions of the question the velocity  $V$  and condensation  $\sigma'$  pertain only to the first hemispherical surface, we must have  $\sigma' = 0$  and  $V = 0$  where the angle  $\theta$  is  $\frac{\pi}{2}$ . Consequently  $F(t) = 0$ . Hence, if we omit periodic terms and those which as containing the factor  $\frac{b^2}{\lambda^2}$  are too small to be significant, the foregoing equation gives, to the second order of approximation relative to the constant  $m$ ,

$$a^2 \sigma' = - \frac{V^2}{2 \kappa^2} = - \frac{\pi^2 k^2 n^2 b^4}{2 \kappa^2} T^2 \sin^2 \theta \cos^4 \theta.$$

But  $T^2 = m^2 \sin q(c_0 - \kappa a t) = \frac{m^2}{2} (1 - \cos 2q(c_0 - \kappa a t))$ , terms containing higher powers of  $m$  than the second being omitted.

Suppressing also the periodic term in this value of  $T^2$ , we finally obtain

$$a^2\sigma' = -\frac{\pi^2 k^2 n^2 b^4 m^2}{4\kappa^2} \sin^2 \theta \cos^4 \theta.$$

The whole pressure on the hemispherical surface estimated in the direction from O towards C is  $2\pi b^2 \int a^2 \sigma' \sin \theta \cos \theta d\theta$ , taken from  $\theta=0$  to  $\theta=\frac{\pi}{2}$ . The result is,

$$\text{Pressure} = -\frac{\pi^3 k^2 n^2 b^6 m^2}{48\kappa^2}.$$

18. Combining with this pressure that which was found in art. 16 to be due to condensation, apart from the effect of constrained motion along the surface of the sphere, the whole pressure, or moving force, in the direction of the propagation of the waves is

$$\frac{\pi^2 knb^4 m^2}{8\kappa^4} - \frac{\pi^3 k^2 n^2 b^6 m^2}{48\kappa^2}, \text{ or } \frac{\pi^2 knb^4 m^2}{8\kappa^4} \left(1 - \frac{\pi knb^2 \kappa^2}{6}\right).$$

It is important to remark here that all the literal quantities in the second term within the brackets have fixed values, if the radius of the sphere be given, excepting the arbitrary factor  $k$ . If the sphere be supposed to be a physical atom, and the unit of space be not larger than the ten-millionth of a square inch, the area  $\pi b^2$  referred to that unit would still be an exceedingly small fraction. It follows therefore that  $kn$  may be an exceedingly large number, and at the same time the term  $\frac{\pi knb^2 \kappa^2}{6}$  be less

than unity. In that case the pressure on the sphere would be in the direction from O towards C, which is that of the propagation of the waves, and would correspond to *repulsion*. But as  $k$  is a quantity entirely arbitrary, it may be supposed to receive a value which would make that term greater than unity; in which case the resulting pressure would be in the opposite direction, and would correspond to *attraction*.

19. These conclusions completely justify the theoretical views respecting *atomic repulsion* and *molecular attraction* which I communicated in an article on "A Mathematical Theory of Heat," contained in the Philosophical Magazine for March 1859. Similar views are advocated in the theories of heat and of molecular attraction proposed in the 'Principles of Physics,' pp. 459-465; but the reasoning is there vitiated by the admission of the hypothesis (subsequently rejected) that the condensation emanating from a centre varies inversely as the square of the distance from the centre instead of inversely as the distance. In the article above mentioned I

have argued more correctly as follows:—"If we suppose waves of condensation to be propagated from all the atoms contained in a spherical space of radius  $r$ , the condensation resulting from the composition of the waves at any distance  $R$  from the centre of the sphere very large compared with  $r$ , will vary [*quam proxime*] as the number of atoms—that is, as  $r^3$  directly, and as  $R$  inversely [the atoms being supposed to be evenly distributed over the spherical space]. If, now, we take another sphere of larger radius,  $r'$ , and another point whose distance,  $R'$ , from the centre of the sphere is such that  $\frac{r}{R} = \frac{r'}{R'}$ , then the condensation at the first point is to the

condensation at the other as  $\frac{r^3}{R}$  to  $\frac{r'^3}{R'}$ ; that is, as  $r^2$  to  $r'^2$ , or as  $R^2$  to  $R'^2$ . Hence, however small may be the condensation propagated from a single atom, the resulting condensation from an *aggregation* of atoms in a spherical space may be of sensible magnitude at considerable distances from the centre, if the number of atoms in the given space be very great. The dynamical effect of these compound waves on a particular atom may be investigated in the same manner as the effect of waves propagated from a single atom. . . . In this manner it is conceivable that while the individual atoms are repulsive to each other, an aggregation of atoms may give rise to a controlling attraction."

20. The assertion in the last sentence of the above quotation admits of being confirmed as follows by results obtained in the present communication. The waves emanating from a *single* atom may be considered to be composite waves, as being the result of the reactions at its surface consequent upon the incidence thereon of waves from all surrounding atoms. Now it may well be supposed that the value of  $kn$  for these composite waves at a distance from their origin equal to that of one of the nearest atoms, is not of sufficient amount to make the expression in art. 18 for the pressure on this atom a negative quantity. Accordingly adjacent atoms will be mutually repulsive. If we next take a *group* of atoms evenly distributed in a certain spherical space, it is supposable that the spherical waves resulting from the composition of the waves from all the atoms in the space may still act repulsively on the atoms situated at the boundary, and that thus there will only be repulsive action between the atoms throughout the spherical space. It is certain, however, from what is shown in art. 19, that, on increasing still further the magnitude of the space, a value of  $kn$  will be reached which corresponds to *attractive* action on an atom at the boundary, and

that consequently beyond a certain limit of magnitude the attractive action at the boundary of the spherical space may exceed the repulsive action. In that case the atoms of the space may be conceived to be divided into component *groups*, the atoms of each of which are mutually repulsive as far as depends on the dynamical effect of their proper waves; and it is evident that under these circumstances the groups will also be mutually repulsive. Those groups situated near the centre of the spherical space will be kept in equilibrium in great measure by their mutual repulsions; but in proportion as a group is further from the centre it will be acted upon in the direction from the centre by a residual repulsion which requires to be counteracted by attractive action towards the centre. The latter force is supplied by the waves composed of the individual emanations from the whole number of atoms, for which waves  $kn$  will be a very large number. The least number of atoms of a given substance which constitute a mass which is thus *self-contained* may be called a *molecule* of that substance. It has long appeared to me that the received mode of attributing attractive and repulsive forces to occult virtues emanating from particles or points leaves wholly unexplained why some forces are repulsive and others attractive. This fact is fully explained by the present theory, which accounts for such diverse action of forces by modes of pressure of the ætherial medium.

21. I do not propose to discuss here the effect of the incidence of a series of waves on a *movable* sphere. This is the case of Example VII. in the 'Principles of Physics,' a solution of which to small quantities of the first order is given in pages 296–298. Another solution, including terms of the second order, which is attempted in pages 446–452 of the same work, I subsequently found to be faulty on account of its involving the supposition of uncompounded plane-waves. I believe, however, that the conclusion arrived at in p. 452, namely that the accelerative action on a movable sphere has a constant ratio to that on a fixed sphere, admits of being satisfactorily established.

22. The atomic and molecular forces to which the foregoing theory applies are usually said, on experimental grounds, to have sensible effects only at insensible distances from their centres of emanation. This description is in agreement with the present theory, as will appear from the following considerations. In the expression obtained in art. 18 for the whole pressure on a single atom resulting from the incidence upon it of the waves from all the atoms of a given spherical group, we may suppose the value of  $m$ , at any distance  $R$  from the

centre of the group large compared with its radius, to be  $\frac{\mu}{R}$  very nearly,  $\mu$  being a certain constant. In case the pressure act repulsively, the quantity in brackets will be positive; and it may readily be shown that the repulsion is a maximum if  $\pi knb^2$  be equal to  $\frac{3}{\kappa^2}$ , which is equal to 2 nearly, the numerical value of  $\kappa$  being, according to theory, 1.2106 ('Principles,' p. 224). Hence that maximum repulsion is

$$\frac{2\pi\mu^2}{16\kappa^4} \frac{b^2}{R^2}, \text{ or } \frac{b^2}{R^2} (0.1828 \mu^2) \text{ nearly.}$$

It will thus be seen that on account of the exceedingly minute value of  $b$  the radius of an atom, this repulsion is of insensible magnitude at very small distances from the centre of the group. If we now suppose the atoms of the group to constitute a *molecule*, the numerical quantity  $kn$  would be very much larger, and the factor  $1 - \frac{\pi knb^2\kappa^2}{6}$  might become a large negative quantity. Yet, as the expression for the attractive action thus indicated would still contain the factor  $\frac{b^2}{R^2}$ , and it is known that the diameter of a molecule must be a very small quantity, the attraction would become insensible at very minute distances from the centre of the molecule, although the sphere of sensible activity would be very much larger than in the preceding case of maximum repulsion. Accordingly in all cases repulsion is controlled by attraction (with the exception, under certain conditions, of the atomic repulsion of gases); and the theory thus accounts for an excess of attraction above repulsion at the boundaries of bodies, such as that to which the phenomena of *capillary attraction* have been attributed.

23. On proceeding to the consideration of spherical atomic groups of larger magnitude than those the attractive action of which suffices to make a molecule self-contained, special hydrodynamical circumstances have to be taken into account, the discussion of which is necessary as a preliminary to the hydrodynamical theory of the *attraction of gravity*. It has been shown (art. 19) that the intensities of the composite waves originating in two spherical atomic groups are to each other, at distances  $nR$ ,  $nR'$  from their centres which are large multiples of their radii  $R$  and  $R'$ , in the proportion of  $R^2$  to  $R'^2$ . Hence it follows that the intensity might be indefinitely increased by increasing the magnitude of the atomic



group ; and it might be supposed that the attractive action would be proportionately increased, so as to be capable of producing sensible effects at distances from the boundaries of bodies much beyond the sphere of activity of capillary attraction. This, as matter of experience, is not the case ; and the fact that such effects are not perceived is theoretically accounted for as follows :—

24. It is to be considered, first, that the motions resulting from the composition of all the minor waves must consist of *harmonic* vibrations, because the opposite motions of any particle towards and from the centre of the waves must be equal—and, again, that the value of  $\lambda$  for the breadth of these composite waves is greater as the number of the components, and consequently the intensity of the compound wave, is greater. Although as respects the ætherial vibrations concerned in atomic repulsion and molecular attraction the excursus of an ætherial particle might not exceed, and might even be much less than, the diameter of the atom on which they are incident, in the case of the much larger vibrations compounded as above stated the excursus of the particle must be supposed very much to exceed that diameter. In fact the total motion would in this case approximate to that of a *stream* in the direction of the propagation while the condensed half of the wave is passing the fixed sphere, and of a stream in the contrary direction while the rarefied half is passing. Now it is known that if a uniform steady stream be incident on a fixed sphere, the consequent condensation at the surface of the sphere will be symmetrical with respect to a transverse plane passing through its centre, and that thus there will be no accelerative action on the sphere. Accordingly it may be supposed that the two streams above mentioned produce separately little effect, and that, as they flow in opposite directions, the residual effect is extremely small. This will account for the *molecular* attraction of a very large group of atoms not being proportionate to the magnitude of the group.

25. There is, however, a residual effect, which, in fact, is the *attraction of gravity*, the origin and amount of which may be ascertained by reference to Poisson's well known solution of the problem of the resistance of the air to a ball-pendulum. According to that solution the ball at a given instant is communicating to the air in the direction *opposite* to that of its motion a momentum equal to a mass of air of its own size multiplied by its velocity at that instant. By analogous reasoning it is proved, in the case of waves incident on a fixed sphere, that there is communicated to the fluid by the reaction of the sphere at a given instant, *in the direction of the motion*

of the fluid, a momentum equal to a mass of fluid of the density of the contiguous fluid and of the size of the sphere, multiplied by the velocity with which the fluid at that instant crosses a transverse plane through the centre of the sphere ('Principles of Physics,' p. 284). Hence, if  $M$  be the magnitude of the sphere,  $\rho_1(1+\sigma)$  the density of the fluid, and  $V$  its velocity at the above mentioned plane, the momentum is  $M\rho_1(1+\sigma)V$ . The corresponding quantity for the rarefied half of the wave is  $-M\rho_1(1-\sigma)V$ . The sum of these is  $2M\rho_1\sigma V$ , which, since  $V = \kappa a \sigma$  nearly, is equal to  $\frac{2M\rho_1 V^2}{\kappa a}$ . This

is the impulse given to the fluid at the given instant in the direction of propagation, and measures the reaction of the fluid on the sphere in the contrary direction. Let  $V = m \sin\left(\frac{2\pi\kappa at}{\lambda} + C\right)$ . Then  $V^2 = \frac{m^2}{2}\left(1 - \cos\left(\frac{4\pi\kappa at}{\lambda} + 2C\right)\right)$ .

Omitting the periodic part, we shall have for the constant part of the impulse, or moving force, towards the origin of the waves  $\frac{M\rho_1 m^2}{\kappa a}$ . If the mass of the sphere be  $M\Delta$ , the

accelerative force is  $\frac{\rho_1 m^2}{\Delta \kappa a}$ , which, it should be observed, is

*independent of the magnitude of the sphere.* This result expresses the force of gravity as due to the attractive action of a molecule of a higher order as to magnitude than the molecule of molecular attraction. For distinction, a molecule of this superior order might be called a *gravity-molecule*. Its magnitude may still be considered to be so small that in comparison with the magnitudes of terrestrial and cosmical masses it may be treated as an infinitesimal quantity. Hence, since according to the theory the attraction of each gravity-molecule is independent of the attraction of all others, the process by which the calculation of the attraction of masses will have to be performed is in accordance with the usually received principles. The factor  $m$ , relative to a given series of waves, varies inversely as the distance from their origin, so that  $m^2$  varies inversely as the square of the distance, which is the law of gravity. It is particularly to be noticed that whereas the expressions for atomic repulsion and molecular attraction involve the magnitudes of the atoms, the force of gravity accelerates equally atoms of different sizes, which accounts for all bodies descending towards the earth with the same velocity when acted upon by the earth's attraction. Lastly, it is to be noticed that, on account of the large value of  $\lambda$  for gravity-waves, they do not suffer sensible retardation or refraction in

passing through gross bodies. I have on several previous occasions treated of the problem of gravitating force theoretically, and by slow steps have approximated to its solution; but before the present attempt, I had not succeeded in exhibiting satisfactorily the *rationale* of this kind of attraction by vibrations.

26. In concluding this communication, I beg to call the attention of mathematicians to the different applications I have made of those laws of the mutual action of the parts of an elastic fluid which for brevity I call *spontaneous vibrations*. The mathematical theory of these vibrations I gave first in the *Philosophical Magazine* for May 1849; afterwards I applied them in accounting for the *polarization* of light in the undulatory theory (*Philosophical Magazine* for December 1852, and 'Principles of Physics,' pp. 331-338); and in the present communication I have shown that they constitute an essential part of the *à priori theory of music*, and that it is necessary to introduce them in hydrodynamical theories of *atomic repulsion* and *molecular attraction*. Apart from the argument which I have elsewhere insisted upon, that these vibrations have been arrived at by *logical* deduction from the premises of hydrodynamics, the fact of their being capable of application in theories so various in kind and important in character as those above mentioned ought, I think, to be regarded as confirmatory of their truth.

Cambridge, July 22, 1876.

#### XXIV. *On some Phenomena of Induced Electric Sparks.*

By S. P. THOMPSON, B.A., B.Sc.\*

[Plate II.]

1. **T**HE observations communicated in this paper are the result of an investigation into the phenomena discovered in November 1875, in Newark, N. J., by Messrs. T. A. Edison and C. Batchelor, and alleged to demonstrate the existence of a new form of energy. A detailed memoir of the phenomena has since appeared in the 'Quarterly Journal of Science' †, by Dr. G. M. Beard, of New York, who accepts, with some hesitation, the hypothesis of a new force.

2. When a galvanic current passing through the coils of an electromagnet is rapidly interrupted, minute bright sparks can, at the moment of interruption, be drawn from the electro-

\* Communicated by the Physical Society.

† "The newly-discovered Force," by George M. Beard, A.M., M.D., New York (*Quarterly Journal of Science*, No. L. April 1876).

magnet-core by an earth-wire or by any conducting substance held in the hand and presented to the core\*. These sparks are best seen when the electromagnet-coils, interrupter, and battery-cells are completely insulated from the ground. The substitution of a bar of any metallic substance, or even carbon or wood coated with tinfoil, in place of the iron core of the electromagnet, does not prevent the production of the spark. With bars of zinc or cadmium, or, better still, of either of these metals amalgamated on the surface, the sparks are brighter.

The minuteness of the sparks makes observation of them a matter of some difficulty; between two points of graphite pencils in a dark box they are more readily seen. They are conducted by all metallic bodies, and by the body; and even pass into insulated conductors of large surface, apparently requiring no completion of the circuit. This circumstance, however, as well as the supposed inability of the sparks to produce any indications upon the galvanometers and electroscopes of ordinary use, may be explained by a further knowledge of their nature. It was this apparent lack of polarity in the nature of the discharge that led to the adoption of the hypothesis of a new form of energy; and the assumption seemed further to be borne out by an apparent absence of physiological effect.

Two forms of the apparatus at first employed to produce the sparks in question are given in figs. 1 & 2. In the first case, where the circuit was rapidly interrupted by an automatic vibrator, the sparks were drawn from the electromagnet-core or from the vibrating armature. In the second they were drawn from a bar of metal placed above, but not in contact with, an electromagnet in a circuit completed by a depressor key.

3. Careful repetition of the experiments failed to elicit evidence of polarity in the spark, or to procure response to the ordinary electrical tests. And the wish to obtain further information upon the nature of this peculiar discharge by studying its effects in illuminating the rarefied gases of vacuum-tubes, rendered it necessary to seek a means of more plentiful production, and to abandon the methods of the original investigators already described; for the sparks so

\* As an interrupter almost any form will answer; a Morse key is good for slow interruptions by hand; or an automatic vibrator, similar to the "break" of an induction-coil may be employed. Similar sparks have doubtless been often noticed by those who have used any form of apparatus involving rapid interruption of the current, and as often attributed to defective insulation. The writer of this paper observed the same thing some years ago in a trembler of particularly good construction, and in vain rewound the coils in hope of eliminating the supposed leak.

obtained had never exceeded the quarter of a millimetre in length, and were quite incapable of producing a luminous discharge through a small vacuum-tube.

The following experiments led to the method desired:—

A rod of soft iron 20 centims. in length was wrapped in folds of vulcanized sheet rubber to a thickness of 2 centims. Around the exterior of this 150 centims. of stoutly insulated copper wire were wound. One end of this wire was connected with one terminal of the secondary wire of a small induction-coil\*, the other end was brought within 15 millims. of the other terminal. The intent of this arrangement was to pass, as in the previous experiments, an intermittent current around a metallic conductor. When the coil was set in action the secondary currents passed with rapid discharge across the interval of about 15 millims. between the end of the wire and the terminal; and it was then found that sparks of several millimetres length could be drawn from the iron rod within the folds of vulcanized rubber by an earth-wire, or by the finger, or by any conductor held in the hand. It was found impossible to convey any charge from the iron rod by a proof-plane to an electroscope; and on leading a wire to a sensitive long-coil galvanometer by Colladon no deflection was obtained, though the sparks were drawn from the wire beyond the galvanometer, through which the force thus manifesting itself must therefore have passed. Identical effects followed the substitution of bars of copper, cadmium, and zinc for the iron rod.

It was next ascertained that no perceptible difference of potential existed between the two ends of the metal rod, and none between the ends and the median portion. The spark could be drawn indifferently at either end, or at the middle, but not from two points at once; nor could it be led by an insulated wire from one point of the bar to another. Other metallic objects in the neighbourhood of the bar became similarly affected; and from them sparks could be drawn. These effects were augmented when the distance of 15 millims. over which the secondary current of the coil discharged itself as a spark was increased to 20 millims. They diminished when the distance was reduced to 5 millims., and ceased entirely when the interval was reduced to nothing, so that the secondary current of the coil passed along unbroken metallic connexions. The conclusion to which these results pointed

\* This coil, capable of throwing sparks of 50 millims. length with two cells of Grove's element, was employed in most of the succeeding experiments. Each piece of apparatus used, including the cells of the battery, was carefully insulated from the table.

was apparently that the effect was one of surface-charge. This surmise might be directly tested by increasing the size of the metallic core and the number of surrounding turns of wire.

4. The arrangement next to be described\*, and shown in diagram at fig. 3, was therefore directly suggested by the preceding experiments. It remained practically unchanged in the subsequent investigation. A large Leyden jar was taken and its inner and outer surfaces joined by a metallic wire, constituting it simply a conductor of large surface. Around it were wrapped sheets of vulcanized rubber to a thickness of 25 millims. About this again were coiled about 30 metres of stoutly insulated copper wire†. As before, one end of this wire was led to the terminal T' (fig. 3)—the other end, *w*, being placed near the terminal T, and so adjusted that the distance *w* T might be increased or diminished at will.

5. When the coil was set in action, sparks could be drawn by an earth-wire, C, from the knob of the accumulator, Q. When the discharging-distance of the coil *w* T was less than 5 millims., the sparks drawn from Q were minute, not exceeding 1 millim. in length, destitute of perceptible physiological action, producing no deflection when led through an ordinary needle-galvanometer, and, as before, were incapable of imparting any charge to a proof-plane.

When *w* was removed to a distance of 10 millims. from T the sparks at Q were larger and brighter; when the distance was increased to 20, 25, and 30 millims. successively, the sparks drawn at Q were much more vivid and exceeded 10 millims. in length. These sparks, though producing comparatively little effect in contracting the muscles of the arm, were excessively painful and penetrating to the nerves of the skin. Their light was brightest when they passed between

\* It will be plain, from the subsequent details, that this arrangement plays the part of an accumulator or condenser, the interior surface of which is represented by the Leyden jar, the exterior being represented by the wire coiled outside. A Leyden jar is not essential; any cylindrical conductor is as good if of large surface; and a sheet of tinfoil laid upon the exterior of the insulating material is more effective than 50 metres of covered wire wound round it. The precise form of the accumulator is immaterial; but thorough insulation must be secured. The above arrangement is preferred because its parts are analogues of those of the experiments that suggested it. The knob of the jar is moreover convenient for drawing sparks from.

† To avoid confusion, in fig. 3 the condenser arrangement is given in diagram, D standing for the insulating folds of rubber, P for one condensing surface (the external wire coils), Q for the other condensing surface (the Leyden jar, which acts in this combination as if made of one piece of metal).

poles of zinc, mercury, or carbon. Their brilliancy was augmented and their length was increased to above 20 millims. when *w* was withdrawn so far from *T* that only a faint brush discharge passed across from *w* to *T*, a distance of about 50 millims.; and a still greater effect was produced when *T* was directly connected with earth. This modification virtually changed the arrangement to that indicated in fig. 4\*.

6. When the spark obtained between *Q* and the earth-wire *C* exceeds 4 millims. in length, it is capable of illuminating a small vacuum-tube. The illumination differs, however, in a remarkable manner from that produced by the ordinary direct discharge of the induction-coil. Instead of the usual phenomena of a small brush of light at the positive pole, and a bright aureole or glow surrounding the negative pole, the illumination is symmetrical, alike at each pole, having both brush and glow at each, and is unaltered by reversing the primary circuit of the coil. An occasional flash, however, seemed to glow at one pole or at the other.

On making single contacts by holding the break-piece of the induction-coil in the fingers the effect was as follows:—at each make-of-contact no illumination; at each break-of-contact an illumination of the same symmetrical nature, affording no evidence of polarity of discharge in one direction more than in another.

7. To ascertain whether the discharge at the two poles was simultaneous or alternating, a rotating mirror was placed beside the vacuum-tube. The images were found to be, roughly speaking, alternate, the discharges being alternately from either pole. The irregularity of the alternation appeared to be dependent partly upon the distribution of the resistances in the circuit, partly upon the irregularities in the vibrations of the break. The occasional flashes of light at one pole or the other were explained by the occasional appearance in the mirror of two contiguous images having the same position, indicating the occurrence of two consecutive discharges in the same direction.

With rotating slits the same phenomenon of reciprocating discharge was observed, though less advantageously.

\* Since the communication of these experiments to the Physical Society, my attention has been called to an article in the *Journal of the Franklin Institute* for June 1871, by Prof. E. J. Houston, of Philadelphia, where, under the title of "A New Connexion for the Induction-coil," he describes a somewhat similar arrangement, in which an insulated conductor is employed in connexion with the coil. With this combination Prof. Houston observed some curious effects of induction in neighbouring bodies; but he seems to have thought them due to escape of electricity, and apparently had no suspicion of the real nature of the sparks so produced.

8. One terminal of a long-coil Thomson's reflecting galvanometer, by Elliott Bros., was put to earth, the other being connected with Q (fig. 3). When the distance  $wT$  was less than 5 millims. no deflection was produced upon the needle and mirror, a tremor being the only result. But when the distance  $wT$  was gradually increased, and the secondary discharge had consequently to accumulate upon its conductors to acquire sufficient density to overleap the resistance of the increased thickness of air between  $w$  and  $T$ , the tremulous motion of the galvanometer-needle became much more marked, the spot of light wandering about on either side the zero of the scale, never at rest, but showing no permanent deflection. When the distance  $wT$  was increased to nearly 50 millims. the brisk flow of sparks gave place to the brush discharge, varied only by the passage of an occasional spark. In this case the wanderings of the spot of light on the scale were still more exaggerated, the needle apparently being turned little by little with an increasing deflection during some twenty or thirty vibrations of the contact-breaker of the coil, then receiving a sudden impetus in the opposite direction as the secondary spark discharged across the interval  $wT$ . This result was entirely confirmatory of the occasional flashes previously observed in the vacuum-tube, with which, on further examination, the more violent oscillations of the galvanometer-needle were found to be simultaneous. The experiment also accords with the previous observations of physicists with respect to the oscillations of the induced currents of the coil. It explains, too, why with an ordinary needle-galvanometer no deflection had been obtained with the minute sparks yielded by the earlier experiments.

9. Having again diminished the distance  $wT$  to 5 millims., a wire was led from Q to the knob of a gold-leaf electroscope. No divergence of the leaves took place; but on drawing sparks by the hand or by an earth-wire from the electroscope knob, from Q, or even from  $T'$ , a divergence of the leaves took place, rapidly followed by collapse at every spark so drawn, the gold leaves being thrown into a state of violent flutter. If the hand, or earth-wire, were suddenly removed from contact, a residual charge usually remained upon the leaves of the electroscope; and this charge was variously positive or negative, changing sign apparently according to the last oscillation of the secondary discharge.

Experimentation with the electroscope could not be carried on when  $w$  was removed more than 10 or 12 millims. from  $T$ , as the momentary charges received by the gold leaves were so violent as to destroy them.



10. Throughout the above experiments the most curious effects of induction were exhibited by all metallic bodies in the neighbourhood of the apparatus. From any of these within a range of several yards sparks of a similar reciprocating nature could be drawn by bringing near an earth-wire or any conductor of large surface, whether insulated or not.

11. It was found, as in a preceding case stated at the close of § 3, that no sparks could be drawn from Q (fig. 3) when  $w$  was brought up to actual contact with T. That is, an intermittent current (as is that in the secondary wire of the coil) passing through a *closed* circuit does not evoke that kind of disturbance in a neighbouring conductor which is evoked when the intermittent current has to overleap an interval of air, however short. This result proved that the appearance of a momentary charge upon Q when  $w$  and T were separated was the result of induction from a momentary charge upon P, and not merely from a current passing through P. The well-known phenomenon of the statical charge produced upon the separated terminals of the secondary circuit of an induction-coil will account for the presence of single momentary charges upon P and upon T. These charges must accumulate through a short but definite period of time until they have acquired sufficient density to overleap with a spark the resistance offered by the layer of air  $w$  T—the density of the accumulating charge depending on the dryness of the atmosphere, the perfectness of the insulation, and the distance between  $w$  and T. Though this period be very short, such charges will necessarily, by the inductive influence of their presence, disturb the electrical equilibrium of all near conductors such as Q. The equally well-known phenomenon of the return-stroke, investigated by Lord Mahon in 1779, will account for the appearance of a return-spark after an interval perhaps not exceeding the thousandth part of a second, thus establishing the reciprocating nature of the discharges passing between Q and C.

12. In the case of the earlier experiments (§ 2), where an interrupted primary current passes round an electromagnet, similar conditions virtually exist. The extra-current consequent on breaking the primary is a current of high electromotive force, like the secondary current of the coil. It, too, has a sensible interval to overleap, though the distance in this case is not constant. It, too, must accumulate upon its conductor in order to acquire sufficient density to overleap the gap in the circuit. It, too, can therefore, during a distinct minute period of time, analyze the neutral electricity of the electromagnet-core across the dielectric of silk or cotton that

insulates the conducting wire therefrom. In this case, too, we have consequently the conditions of a rapidly charged and discharged condenser, giving rise to reciprocating induced sparks.

13. This conclusion can be directly proved by recurring to the earlier experiments (fig. 2) and introducing across the circuit a wire of suitable resistance to short-circuit the extra-current so that it may complete its route without having to overleap the break at the key. This is shown in diagram in fig. 6, where R is a box of resistance-coils introduced between  $a$  and  $a'$ . If the wire of which the electromagnet E is made be stout, its resistance will be a mere trifle, and a resistance of 3 or 4 ohms at R will be sufficient. In some cases a drop of pure water between the points of contact at K has also the result of obviating the extra-current spark. And in every case, when the conditions are so arranged as to obviate the appearance, on breaking contact, of the extra-current spark at K, no earth-wire, nor any conductor, will draw a spark from the electromagnet-core—a decisive proof that the sparks so drawn were due to induction by the statical charge of the accumulating extra-current, and to that alone.

XXV. *On the Nebular Hypothesis.*—IV. *Correlations of Central Force.* By PLINY EARLE CHASE, LL.D., S.P.A.S.,  
Professor of Philosophy in Haverford College.

[Continued from p. 36.]

THE various accordances of planetary mass and position which have been pointed out in preceding papers, the harmonic prediction of a source of planetary disturbance at .269 Earth's radius vector which was confirmed by the sun-spot observations of Messrs. De La Rue, Stewart, and Loewy, the comparison of gravitating and chemical force by means of the explosive energy of  $H_2O$ , the measurement of gravitating waves, the agreement between the ratio of selenium-conductivity in sunlight to its conductivity in darkness and the ratio of Sun's light-emitting to its earth-attracting energies, and my various mechanical estimates of solar mass and distance seem to be dependent upon mathematical laws, among which the following merit special consideration.

Let  $f$  = velocity communicable, at distance  $r$ , in time  $t$ , by

any central force varying as  $\frac{1}{d^2}$ ,

$f_1$  = velocity communicated by a single impulse, or in a single instant,

$f_n = nf_1$  = velocity communicated by  $n$  equal impulses  
or in  $n$  instants, the distance remaining constant.

Then

$\alpha$ . If the pressure resulting from  $f$  is constant, it must either be exactly counterbalanced by some opposing force or forces, so as to produce relative rest, or the motion must be maintained at a constant distance from the centre, so as to produce circular revolution, or, if the opposing force results from the transformation of interfering revolutions, the two conditions may be combined so as to produce rotation. The velocity of circular revolution =  $\sqrt{fr}$ ; the velocity of rotation =  $\frac{\sqrt{fr}}{n}$ .

The value of  $n$  may be found by experiment; or if the aggregating medium is homogeneous, it may easily be calculated.

$\beta$ . If the pressure is varying, and so exerted as to produce radial motion directly towards the centre, the velocity acquired at any given distance  $\rho$  may be found by the equation  $v_\rho = \left\{ \frac{2fr(r-\rho)}{\rho} \right\}^{\frac{1}{2}}$ . At the centre,  $\rho=0$  and  $v_\rho = \infty$ .

$\gamma$ . If the pressure leads to perpetual radial oscillations synchronous with perpetual circular oscillations under constant pressure, the range of the radial oscillations from the centre is  $2r$ , and the mean velocity of radial oscillation is  $\frac{2}{\pi} \sqrt{fr}$ . The equation  $\frac{2}{\pi} \sqrt{fr} = \left\{ \frac{fr(2r-\rho)}{\rho} \right\}^{\frac{1}{2}}$  gives  $\rho = \frac{2\pi^2 r}{\pi^2 + 4} = 1.4232 r$ .

$\delta$ . The *vis viva* of the total force varying as  $\frac{1}{\rho}$ , the theoretical *vis viva* of average radial velocity : *vis viva* of synchronous circular velocity :: 1 : 1.4232.

$\epsilon$ . By the dynamic theory of gases, the average radial velocity = velocity of constant volume; by the laws of thermodynamics, heat varies as *vis viva*; and we have already seen ( $\alpha$ ) that the velocity of circular revolution = velocity of constant pressure. We have thus a mathematical verification of the empirical ratio between heat of constant volume and heat of constant pressure.

$\zeta$ . If the constrained synchronous rotation of particles in a spheroid, and the free revolution of exterior particles under the influence of any central force whatever, are due to the same primitive impulses ( $\alpha$ ), the velocity of those impulses is the limit of velocity towards or from which both motions tend. Let, therefore,  $v_f = \sqrt{fr}$  = velocity of free revolution, varying as  $\sqrt{\frac{1}{r}}$ ,  $v_n = \frac{\sqrt{fr}}{n}$  = constrained velocity at same point, or

velocity of superficial equatorial rotation varying as  $\frac{1}{r}$ ; then

$v_0 = [v_i] = [v_{ii}] = nv_i = n^2 v_{ii} = n\sqrt{fr} =$  limit of tangential velocity, both of revolution and of rotation, under a reduction of spheroidal volume to an equatorial radius  $= \frac{r}{n^2}$ . Under such reduction all the particles in the equatorial planes would have the velocity of free revolution, or of perfect fluidity, provided the sphere were homogeneous.

$\eta$ . If contraction were to go still further, the centrifugal force of rotation would give the nucleal particles orbits of increasing ellipticity. Those orbits would finally become linear, leading to complete dissociation when the mean linear velocity  $\frac{2}{\pi}[v_{ii}]$  was sufficient to produce a constant motion with the parabolic perifocal velocity  $[\sqrt{2fr}]$ . These velocities coincide at

$\frac{2r}{n^2\pi^2}$  when  $\frac{n^2\pi^2}{2} \times \frac{2}{\pi} \times \frac{\sqrt{fr}}{n} = \frac{n\pi}{\sqrt{2}} \times \sqrt{2fr} = n\pi\sqrt{fr}$ , thus

showing that the limiting velocity between complete dissociation and incipient aggregation is  $\pi \times$  the limiting velocity between complete aggregation and incipient dissociation ( $n\sqrt{fr}$ ; see  $\xi$ ).

$\theta$ . Let  $t =$  time of describing  $\frac{1}{2}r$  in virtual approach to the centre, under the action of a central force  $f$ ,  $=$  time of describing  $r$  in circular revolution or motion under constant pressure;  $fr = \sqrt{fr}$ ;  $\pi t =$  time of free semicircular oscillation;  $n\pi t =$  time of constrained semirotation;  $n\pi ft = n\pi\sqrt{fr} =$  velocity acquired in the time of semirotation. Therefore the velocity of any central impulses which are capable of producing cosmical aggregation, free revolution, and nucleal rotation  $=$  velocity produced by constant equatorial pressure acting for a half-rotation.

$\iota$ . If the central force is solar gravity, the limiting velocity ( $n\pi\sqrt{fr}$ ) of uniform impulses which would account for solar aggregation, solar rotation, and planetary revolution is *the velocity of light*. For Earth's distance from Sun being 214.86 solar radii, the velocity of light is  $(214.86r \div 497.825) = .4316r$  per sec. If we make this velocity  $= n\pi\sqrt{fr}$ , and find the value of  $\sqrt{fr} = \sqrt{gr} = 2\pi r \times 214.86 \div (365.2564 \times 86400) = .00062705r$  per sec.,  $n = .4316 \div \pi\sqrt{fr} = 219.0894$ , and  $v_{ii} = \sqrt{fr} \div n = .0000028622r$  per sec. The accordance of this value with other estimates ( $\mu$ ) is as follows, the unit being  $r \div 10000000000$ :—

Spörer.	Nebular.	Light.	Schwabe.
29533	29192	28622	28511.

$\kappa$ . In nebular condensation the atmospheric radius varies as  $t^{\frac{2}{3}}$ , while the nucleal radius  $\propto t^{\frac{1}{3}}$ . The nucleal radius therefore varies as the  $\frac{3}{4}$  power of the atmospheric radius. In ordinary considerations of the nebular hypothesis, planetary aggregation has been regarded as atmospheric under a velocity varying as  $\sqrt{\frac{1}{r}}$ ; but there are many traceable evidences of

nucleal influence under a velocity varying as  $\frac{1}{r}$ . Obermayer's law (Trans. Vienna Acad.; 'Nature,' June 1, 1876, p. 119), that the friction-coefficient of permanent gases varies as the  $\frac{3}{4}$  power of that of the coercible gases and as the absolute temperature, seems to be a particular case of the nucleal and dissociating influences of central force.

$\lambda$ . The formation of the Jovian and Telluric belts, as indicated by the limiting velocity of their chief planets, shows the combined action of nucleal and atmospheric influences. At the equator  $g = 32\frac{1}{2}$  ft.; the time of  $\frac{1}{2}$  rotation = 43200 sec.;  $\therefore n\pi\sqrt{fr} = 43200 \times 32\frac{1}{2}$  ft. = 262.5 m. per sec. The limiting velocity for Jupiter is nearly, if not precisely the same; but it cannot be so accurately determined on account of greater uncertainty in regard to the elements of calculation. The solar modulus of light at the principal centre of gravity of our system ( $\odot \mu$ ) is 505400 solar radii; the nucleal formative is  $\therefore \sqrt{505400} \times$  the atmospheric velocity; when the nucleal velocity ( $\iota$ ) was the velocity of light ( $\cdot 4316r$ ), the atmospheric velocity was  $\cdot 4316r \div \sqrt{505400} = \cdot 0006071r$ . If  $\cdot 0006071r = 262.5$  m., Sun's distance ( $214.86r$ ) = 92,902,000 miles.

$\mu$ . Earth was the seat of theoretical primitive nucleal rupture, the velocity of primitive rotation within the nucleus varying as  $r$ , and communicating a similarly varying *vis viva* of superficial rotation to the shrinking nucleus. This gives a present theoretical time of solar rotation = 365.256 days  $\div \sqrt{214.86} = 24.912$  days. The estimates from observation vary between Spörer's (24.624 days) and Schwabe's (25.507 days).

$\nu$ . According to the same data, the present height of possible solar atmosphere, or the radius of equal centripetal and centrifugal solar force, should be  $(214.86)^{\frac{2}{3}}r = 35.873r$ ; the time of planetary revolution at Sun  $31558150$  sec.  $\div (214.86)^{\frac{2}{3}} = 10020.242$  sec.; the present limit of uniform planetary velocity  $2\pi r \div 10020.242 = \cdot 00062705r$  per sec.; the present solar modulus of light  $(\cdot 4316 \div \cdot 00062705)^2 r = 473755.65r$ ; the present modulus-atmosphere  $35.873 \times$  modulus = 16995141  $r$ .

ξ. Undulations in elastic media tend to generate other undulations in arithmetic, geometric, harmonic, and various figurate progressions.

ο. Radial oscillations through  $\rho = mr$  give the central dissociating velocity  $\sqrt{2fr}$  at  $\frac{mr}{m+1}$ .

π. If we take as determinants of a parabola a vertex at  $\frac{1}{\pi}$  solar radius, or the primitive circumference of complete association ( $\eta$ ), and an abscissa at present surface of modular atmosphere ( $\nu$ )  $\chi_{35.873} = 35.873 \times \text{modulus}$ , the general equation  $\chi_n = \xi\eta \pm n\xi^{n^2}$  furnishes 27 abscissas in regular progression, of which 9 are between Sun and Mercury, 9 are between Neptune and α Centauri, and the central 9 represent points of dissociating nebular velocity ( $ο$ ), as will be seen by the following Table :—

$n$ .	Log $\chi_n$ .	Log sec. per.	Log mean.	Log sec. aph.	$mr \div (m+1)$ .
9.87322	1.6774	1.6806	1.7950	1.8855	3.4 8
10.87322	1.9099	1.8586	1.8905	1.9200	4.1 9
11.87322	2.1541	2.1256	2.1561	2.1845	5.2 0
12.87322	2.4102	2.3248	2.3901	2.4468	6.3 1
13.87322	2.6779	.....	.....	.....	7.4 2
14.87322	2.9575	2.9420	2.9692	2.9949	8.5 3
15.87322	3.2488	3.2064	3.2447	3.2799	9.6 4
16.87322	3.5519	3.5217	3.5571	3.5897	10.7 5
17.87322	3.8668	3.8614	3.8678	3.8740	11.8 6
36.87322	7.6622	7.6571	.....	7.6860	12.9 7
					α Cent.

The tabular logarithms for α Centauri (7.6571 and 7.6860) represent respectively the least and greatest estimates that have been made of its distance. The planetary secular perihelia and aphelia are the values computed by Stockwell (Smithsonian Contributions, 232, pp. 37, 38).

ρ. Weber, Kohlrausch, and Maxwell have identified the velocity which connects electrostatic with electrodynamic action with the velocity of light. These accordances extend the mathematical correlation to heat (α to ε inclusive), cosmical and molecular aggregation, dissociation, rotation, and revolution (α to θ inclusive), gravitation (ι to π inclusive), and all central forces (α to δ and ξ to θ inclusive, ξ, ο).

XXVI. *On a repetition of Dr. Kerr's Experiments on a new Relation between Electricity and Light.* By J. E. H. GORDON, B.A., *Gonville and Caius College, Cambridge*.\*.

I HAVE been for some months engaged on a repetition of the experiments described by Dr. Kerr in the *Philosophical Magazine* for November and December 1875. I have as yet been absolutely unable to obtain Dr. Kerr's result; and I think, when I have described the precautions taken, I shall be able to show that either Dr. Kerr's success or my failure is due to some cause not yet noticed by either of us; in any case the cause of the difference in our results will be worth looking for.

*The optical arrangements.*

The polarizing apparatus consisted of a Nicol's prism and collimator, as described by me in my last paper†. The end of the collimator which carried the prism projected through a hole in a screen, which protected the observer from stray light. The collimating-tube was fixed in cement to the brick experimenting-table.

Monochromatic light was used, generally that of a powerful paraffine lamp, coloured red by means of a test-tube containing coloured liquid and hung in front of the prism.

Sometimes a solar spectrum was thrown on a card, a slit in which admitted one colour to the Nicol.

*The analyzing-apparatus.*

This, which was all mounted on a massive block of wood cemented to the brick table, consisted:—

1. Of a Babinet's compensator (kindly lent to me by Professor Maxwell).

2. Of a powerful microscope used with a two-inch power, at the back of which a Nicol's prism was fixed. The objective and prism revolved together in a collar. The microscope and collimator were both horizontal. The diagonals of the Nicols were respectively inclined at  $\pm 45^\circ$  to the horizon; distance from lens of collimator to compensator 2 ft. 6 in.

The bands were very distinct. Their apparent distance from centre to centre, as determined by a camera lucida on the eyepiece, upon a sheet of paper 10 inches distant, was  $1\frac{1}{2}$  inch for red light. For their observation the eyepiece of the microscope was furnished with fine cross-wires.

\* Communicated by the Author.

† Determination of Verdet's Constant. Read before the Royal Society, June 15, 1876.

The breadth of the black bands was about  $\frac{1}{5}$  that of the bright ones.

To test the sensitiveness of the apparatus, a black band was brought upon the cross-wires, and a piece of glass  $\frac{3}{8}$  inch thick and 2 inches square was compressed in the fingers. A very moderate compression served to displace the black band, so that its edge, instead of its centre, came upon the cross-wires, while moderate compression in a small iron screw clamp moved the band, so that instead of the centre of the black band, the centre of the bright band next on the right or left, according as the compression was vertical or horizontal, came on the cross-wire. This corresponds to an apparent displacement of  $\frac{3}{4}$  inch, or a retardation of about  $\frac{1}{2}$  a wave-length.

#### *The electrical arrangements.*

*The coil.*—The electricity was produced by a powerful induction-coil by Apps, giving a thick spark 16 inches long in air. It was provided with a vibrator and a clock contact-breaker, both of which were used in the experiments.

The coil was worked by 9 or 10 quart Grove cells. It was managed by an assistant, while fine wires suspended by silk threads carried the secondary current to the optical apparatus.

The coil was placed behind the observer ; so that his eyes were not dazzled by the sparks.

#### *The experiments.*

A piece of plate glass 1 inch thick and 6 inches square, had holes drilled, as described by Dr. Kerr, in opposite edges, the bottoms of which holes came within  $1\frac{1}{8}$  inch of each other.

Quill glass tubes a foot long were put into these holes nearly to the bottom. Inside the quill tubes were stout brass wires, which reached to the bottoms of the holes.

One tube and wire having been put in and the whole apparatus warmed, it was set on one edge on the table, so that the glass tube stood vertical. A kind of cell was made, consisting of two panes of window-glass pressed against the faces of the plate, and projecting above it, and two blocks of cork, the thickness of the plate, between them above the plate. The thin glasses were *slightly* greased, and the interstices between them and the corks filled up with a little flour-and-water dough. This then formed a cell 2 inches long,  $1\frac{1}{2}$  deep, and 1 inch wide. In the centre was the vertical tube. The cell was now filled with Apps's electrical cement. This is a compound consisting of shellac, &c., used for perforating glass by means of induction-coils. It is so good an insulator that blocks of glass whose diameter is not more than twice their



thickness can be perforated when the points of the secondary terminals are imbedded in it. When hot it is nearly as liquid as water. The tube and wire were taken up and replaced once or twice, so that the liquid cement might fill every interstice. When the cement was hard, the side glasses and corks were removed; and, the plate being reversed, the second conductor was fixed in the same way.

Next day the experiment was tried. The plate was mounted with its faces perpendicular to the line of light, its conductors horizontal, and its position such that the light passed between their points. The coil was then worked. The insulation was so good that, while wires from each pole were connected to the wires let into the glass, sparks of 12 inches long were easily obtained between the terminals. The coil was worked both with the vibrator and with the clock, with currents continued both for short and long periods, currents in both directions both quickly and slowly alternated, and both with and without Leyden jars.

In not one of the experiments was any effect whatever observed on the light. The compensator was then removed, and, the field being darkened, all the above experiments were repeated. No return of the light or any other effect whatever was obtained.

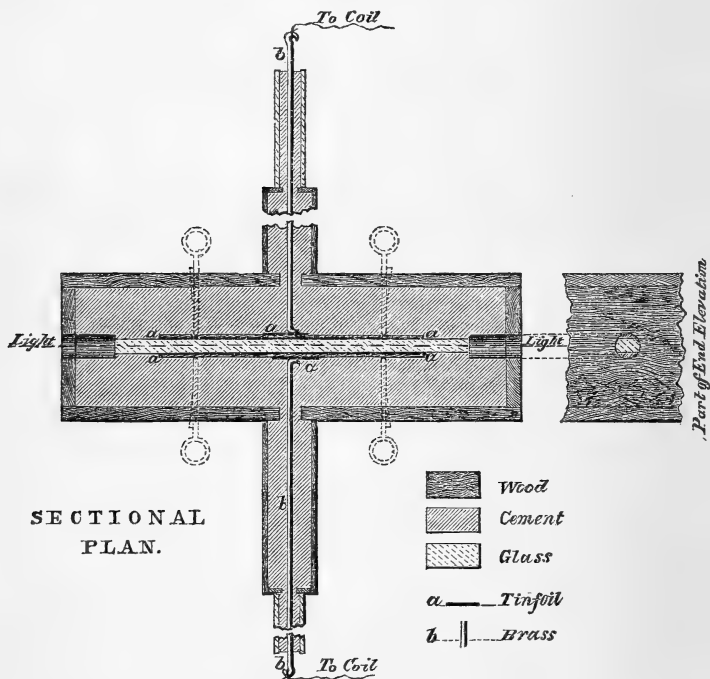
But Dr. Kerr has stated that the electrified glass acts like a crystal. Now we know that the action of crystal on polarized light increases with the thickness through which the light is transmitted. I thought therefore that if, instead of sending the light through 1 inch of electrified glass, I could send it through 15 or 20 inches the effect produced would be so much increased that, if my having been unable to see it in the former case arose from its smallness and not from its absence, I should now be able to see it. I therefore had a piece of fine plate glass prepared, 20 inches by 10 and  $\frac{3}{4}$  of an inch thick, and its ends polished. Light passing in at one end and out at the other had then to pass through 20 inches of glass. I then pasted strips of tinfoil 15 inches long on each side, and placed the glass vertical in the line of light.

I then connected the opposite tinfoils to the secondary poles and worked the coil. No effect was obtained. But only sparks four or five inches long could be used, owing to the electricity springing round the glass.

It was then resolved to insulate the latter so as to allow a greater stress to be applied to it.

For this purpose a box was made of inch mahogany, whose inside dimensions were, length 24 inches, depth 16, breadth 7. Two pieces of barometer-tube being fixed horizontally across

it, 3 inches from the bottom, the glass was set on one of its long edges on them, in the centre of the box. Two holes  $1\frac{1}{4}$  inch in diameter were made opposite to each other in the ends. Into these plugs of mahogany, whose inside ends were padded with leather, were inserted, so that the leather ends pressed on the centres of the end edges of the glass. Holes being made in the outside ends of these plugs, long brass stair-rods were put through them, so that the middles of the rods were in the plugs and the ends projected horizontally in both directions. The ends being tightly drawn together with string, the elasticity of the rods pressed the plugs tightly upon the glass. The plugs and leather were slightly oiled.



Two pieces of tinfoil 15 inches by 5, were pasted on thin slips of soft wood, and placed symmetrically on the sides of the glass. Each was kept in position by two stout pointed wires (kitchen skewers), which were passed through holes in the mahogany, and whose points stuck into the soft wood carrying the tinfoil, and pressed it against the glass. Each skewer was fixed by a nail driven alongside it into the mahogany. At the centre of each side, and at right angles to it, was fixed

a wooden box 3 inches square and 10 inches long ; a lamp-chimney fixed at the end continued the length another 6 inches.

A stout brass wire, protected by a glass tube where it passed through the mahogany, was fixed along the axis of each projecting box and lamp-glass, and pressed against a little bit of tinfoil pasted on the outside of the strip of wood which held the tinfoil against the glass. A short bit of wire let into the wood connected the small bit of tinfoil with the large one. The whole of the box, side boxes, and lamp-glasses were now filled with the cement already described. About 115 lbs. were used ; it was all melted at once, and poured in. As it was not convenient to warm the mahogany case, the cement was heated considerably above its melting-point, so that it might warm all the wood and glass before commencing to set.

While it was setting (which took about 4 days) the stair-rods were every few hours removed from the end plugs, and the latter twisted round a few times, and occasionally removed for a few seconds to be re-oiled.

When the cement was quite set, the plugs were removed, leaving at each end a cylindrical cavity 2 inches deep, at the bottom of which an inch of the edge of the glass could be seen. When these portions of the glass had been cleaned with a little benzine, it was found that an object placed outside one plug-hole could be well seen from the other through the 20 inches of glass.

The skewers which had not been previously touched were now drawn out with some difficulty. The box being laid on its side, one of them previously heated in a spirit-lamp was replaced for a moment, so as to melt the cement at the edges of each hole ; a little fresh cement was now poured in. When it solidified, the holes were completely filled up ; on trying the box with the coil the insulation was all that could be desired. While the poles were connected to the secondary terminals, sparks of 11 and 12 inches were easily obtained between the latter. When the terminals were separated too far for the sparks to pass, the peculiar sound which accompanies the sudden charging of a condenser unable to discharge was heard in the box, showing that neither of the tinfoils had shifted.

The box was now placed between the Nicols, so that the light passed from one plug-hole to another. Every experiment which had been performed with the perforated glass was repeated with the box. Not the slightest effect of any sort or kind was observed on the light.

It is very likely that I have omitted some precautions taken by Dr. Kerr ; but I do not think that I have omitted any described in his paper.

If Dr. Kerr can show me what mistake I have made, no one will be better pleased than myself, as the establishment of such a relation as, according to Dr. Kerr, exists between electricity and light will be a most important step in physical science.

Pixholme, Dorking,  
August 5, 1876.

XXVII. *Note in regard to a Multiple Differentiation of a certain expression.* By J. W. L. GLAISHER, M.A., F.R.S.\*

CONSIDER the multiple definite integral

$$\int_0^\infty \int_0^\infty \int_0^\infty \dots \exp \left( -a_1 x_1^2 - a_2 x_2^2 \dots - a_n x_n^2 - \frac{q}{x_1^2 x_2^4 x_3^8 \dots x_n^{2^n}} \right) dx_1 dx_2 \dots dx_n, \quad (1)$$

where, as is convenient in printing complicated exponentials,  $\exp(a)$  is written for  $e^a$ .

By the aid of the well-known integral

$$\int_0^\infty \exp \left( -ax^2 - \frac{b}{x^2} \right) dx = \frac{\sqrt{\pi}}{2\sqrt{a}} \exp \{ -2\sqrt{(ab)} \},$$

we readily see that the integral (1)

$$= \left( \frac{\sqrt{\pi}}{2} \right)^n \frac{1}{\sqrt{(a_1 a_2 \dots a_n)}} \exp \left( -2^{2(1-\frac{1}{2^n})} \sqrt{a_n} \cdot \sqrt[4]{a_{n-1}} \cdot \sqrt[8]{a_{n-2}} \dots \sqrt[2^n]{a_1} \cdot \sqrt[2^n]{q} \right). \quad (2)$$

Now from an inspection of the integral (1) it is evident that if we denote by  $u$  the quantity (2), then,  $i$  being any integer,

$$\left( \frac{d}{da_1} \right)^i \left( \frac{d}{da_2} \right)^{2i} \left( \frac{d}{da_3} \right)^{4i} \dots \left( \frac{d}{da_n} \right)^{2^i i} u = \left( \int_0^\infty dq \right)^i u; \quad (3)$$

viz. the differential expression on the left-hand side of (3) is equal to the  $n$ th integral of  $u$  with regard to  $q$ . It is clear that the quantity subject to the exponential sign in (2) need not be negative; so that, if we take

$$-2^{2(1-\frac{1}{2^n})} \sqrt[2^n]{q} = \sqrt[2^n]{p},$$

and put  $i=1$  (whereby there is no loss of generality), the theorem is that if

$$v = \frac{1}{\sqrt{(a_1 a_2 \dots a_n)}} \exp \left( \sqrt{a_n} \cdot \sqrt{a_{n-1}} \cdot \sqrt[8]{a_{n-2}} \dots \sqrt[2^n]{a_1} \cdot \sqrt[2^n]{p} \right),$$

\* Communicated by the Author.

then

$$\left(\frac{d}{dp}\right)\left(\frac{d}{da_1}\right)\left(\frac{d}{da_2}\right)^2\left(\frac{d}{da_3}\right)^4\ldots\left(\frac{d}{da_n}\right)^{2^n}v = \frac{1}{2^{2(2^n-1)}}v.$$

As an example put  $n=2$ ; then if

$$v = \frac{1}{a^{\frac{1}{2}}b^{\frac{1}{2}}}\exp(b^{\frac{1}{2}}a^{\frac{1}{4}}p^{\frac{1}{4}}),$$

the theorem is that

$$\left(\frac{d}{dp}\right)\left(\frac{d}{da}\right)\left(\frac{d}{db}\right)^2v = \frac{1}{64}v.$$

Writing, for brevity,  $w$  for  $\exp(b^{\frac{1}{2}}a^{\frac{1}{4}}p^{\frac{1}{4}})$ ,

$$\frac{dv}{da} = w\left(\frac{1}{4}\frac{p^{\frac{1}{4}}}{a^{\frac{5}{4}}} - \frac{1}{2}\frac{1}{a^{\frac{3}{2}}b^{\frac{1}{2}}}\right),$$

$$\left(\frac{d}{da}\right)\left(\frac{d}{db}\right)v = w\left(\frac{1}{8}\frac{p^{\frac{1}{2}}}{ab^{\frac{3}{2}}} - \frac{1}{4}\frac{p^{\frac{1}{4}}}{a^{\frac{5}{4}}b} + \frac{1}{4}\frac{1}{a^{\frac{3}{2}}b^{\frac{3}{2}}}\right),$$

$$\left(\frac{d}{da}\right)\left(\frac{d}{db}\right)^2v = w\left(\frac{1}{16}\frac{p^{\frac{3}{4}}}{a^{\frac{3}{4}}b} - \frac{3}{16}\frac{p^{\frac{1}{2}}}{ab^{\frac{3}{2}}} + \frac{3}{8}\frac{p^{\frac{1}{4}}}{a^{\frac{5}{4}}b^2} - \frac{3}{8}\frac{1}{a^{\frac{3}{2}}b^{\frac{5}{2}}}\right),$$

which differentiated with regard to  $p$  becomes

$$= w \cdot \frac{1}{64}\frac{1}{a^{\frac{1}{2}}b^{\frac{1}{2}}} = \frac{1}{64}v.$$

The theorem is by no means an obvious one; and if the differentiations be performed in the order in which they stand, the verification even for a moderate value of  $n$  is tedious. If, however, the differentiations be performed in a certain other order, each of the intermediate results consists of only a single term, and we thus obtain an elementary and very interesting proof of the theorem, and have, as it were, its *raison d'être* exhibited. The process will be best understood by taking only three letters,  $a, b, c$ , besides  $p$ . Let

$$v = \frac{1}{a^{\frac{1}{2}}b^{\frac{1}{2}}c^{\frac{1}{2}}}\exp(c^{\frac{1}{2}}b^{\frac{1}{4}}a^{\frac{1}{8}}p^{\frac{1}{8}});$$

and for the sake of brevity write  $w$  for  $\exp(c^{\frac{1}{2}}b^{\frac{1}{4}}a^{\frac{1}{8}}p^{\frac{1}{8}})$ , and  $[p], [pc], \&c.$  for  $\frac{dv}{dp}, \frac{d^2v}{dpdc}, \&c.$ ; then, omitting all numerical factors,

$$\begin{aligned}
[p] &= w \cdot \frac{1}{c^{\frac{1}{2}} b^{\frac{1}{2}} a^{\frac{1}{2}}} \cdot \frac{c^{\frac{1}{2}} b^{\frac{1}{4}} a^{\frac{1}{4}}}{p^{\frac{7}{8}}}, \\
[pc] &= w \cdot \frac{b^{\frac{1}{4}} a^{\frac{1}{8}}}{b^{\frac{1}{2}} a^{\frac{1}{2}} p^{\frac{7}{8}}} \cdot \frac{b^{\frac{1}{4}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{c^{\frac{1}{2}}}, \\
[pcb] &= w \cdot \frac{a^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{a^{\frac{1}{2}} p^{\frac{7}{8}} c^{\frac{1}{2}}} \cdot \frac{c^{\frac{1}{2}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{b^{\frac{3}{4}}}, \\
[pebc] &= w \cdot \frac{a^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{a^{\frac{1}{2}} p^{\frac{7}{8}} b^{\frac{3}{4}}} \cdot \frac{b^{\frac{1}{4}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{c^{\frac{1}{2}}}, \\
[pebca] &= w \cdot \frac{p^{\frac{1}{8}} p^{\frac{1}{8}} b^{\frac{1}{4}} p^{\frac{1}{8}}}{p^{\frac{7}{8}} b^{\frac{3}{4}} c^{\frac{1}{2}}} \cdot \frac{c^{\frac{1}{2}} b^{\frac{1}{4}} p^{\frac{1}{8}}}{a^{\frac{7}{8}}}, \\
[pebeac] &= w \cdot \frac{p^{\frac{1}{8}} p^{\frac{1}{8}} b^{\frac{1}{4}} p^{\frac{1}{8}} b^{\frac{1}{4}} p^{\frac{1}{8}}}{p^{\frac{7}{8}} b^{\frac{3}{4}} a^{\frac{7}{8}}} \cdot \frac{b^{\frac{1}{4}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{c^{\frac{1}{2}}}, \\
[pebeacb] &= w \cdot \frac{p^{\frac{1}{8}} p^{\frac{1}{8}} p^{\frac{1}{8}} p^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{p^{\frac{7}{8}} a^{\frac{7}{8}} c^{\frac{1}{2}}} \cdot \frac{c^{\frac{1}{2}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{b^{\frac{3}{4}}}, \\
[pebeacbc] &= w \cdot \frac{p^{\frac{1}{8}} p^{\frac{1}{8}} p^{\frac{1}{8}} p^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{p^{\frac{7}{8}} a^{\frac{7}{8}} b^{\frac{3}{4}}} \cdot \frac{b^{\frac{1}{4}} a^{\frac{1}{8}} p^{\frac{1}{8}}}{c^{\frac{1}{2}}}, \\
&= w \cdot \frac{1}{a^{\frac{1}{2}} b^{\frac{1}{2}} c^{\frac{1}{2}}};
\end{aligned}$$

and the final numerical coefficient of  $w$  is evidently

$$= \frac{1}{8} \cdot \frac{1}{8} \cdot \frac{1}{4^2} \cdot \frac{1}{2^4} = \frac{1}{16384}.$$

In this process we see that in every alternate line the letter  $c$  disappears from the coefficient of  $w$ , that in every fourth line, starting with the second, the letter  $b$  disappears, and that in the fourth line the letter  $a$  disappears; while  $p$  disappears from the final result, and the other letters, each raised to the power  $\frac{1}{2}$ , are left in the denominator.

It is evident that the above procedure is quite general, and that, following the law of succession there indicated, one letter is always absent after each differentiation. To state the law, suppose (for greater convenience) the order of the  $n$  letters reversed, so that

$$v = \frac{1}{a^{\frac{1}{2}} b^{\frac{1}{2}} c^{\frac{1}{2}} \dots k^{\frac{1}{2}}} \exp(p^{\frac{1}{2^n}} \cdot a^{\frac{1}{2^n}} \cdot b^{\frac{1}{2^{n-1}}} \dots k^{\frac{1}{2}});$$

then, the differentiation with regard to  $p$  having been first effected, if we take  $2^n$  compartments and place in every alter-

nate one starting from the end an  $a$ , in every alternate one of those that are vacant beginning with the second a  $b$ , in every alternate one of those that are still vacant, beginning with the fourth, a  $c$ , and so on, we obtain the order in question, which is  
*pabacabadabacabaeabacabadabacabafabaca . . .*

This is of course the binary law ; viz., if we write down in order the numbers in the binary scale 1, 10, 11, 100, 101, 110, 111, 1000, 1001, 1010, 1011, 1100, &c., then every alternate one ends with 1 ; every fourth one, starting with the second, ends with 10 ; every eighth one, beginning with the fourth, ends with 100, and so on ; and the reason is the same in the two cases.

A little thought shows that if in the differentiations this succession be followed, each letter must disappear when its turn comes, and that, after all the operations are performed, each letter except  $p$  will be left in the denominator with exponent  $\frac{1}{2}$ . This and the way in which it happens render the result an interesting one.

As for the numerical coefficient, it is clearly equal to the reciprocal of

$$2^n \cdot (2^n)^1 (2^{n-1})^2 \dots 4^{2^{n-2}} \cdot 2^{2^{n-1}},$$

viz. of

$$2^{2^{(2^n-1)}}.$$

## XXVIII. On Salt Solutions and Attached Water.

By FREDERICK GUTHRIE\*.

[Continued from vol. i. p. 455.]

### V.

#### (1) Separation of Ice, Salt, Hydrate or Cryohydrate from Salt Solutions (continued).

§ 148. Separation of Ice &c. from Solutions of Chloride of Barium.

TABLE XXIX.

BaCl <sub>2</sub> per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidi- fication begins.	Nature of solid.
5	95	— 0·9	Ice.
10	90	— 2·2	"
15	85	— 4·0	"
20	80	— 6·0	"
21·83	78·17	— 7·5	Cryohydrate.
23·98	76·02	0 0	BaCl <sub>2</sub> + H <sub>2</sub> O.
30	70	+25	"

\* Communicated to the Physical Society, June 24, 1876.

I again found the temperature of the cryogen to be  $-7^{\circ}\cdot 3$  ( $-7^{\circ}\cdot 2$ , § 78), and the temperature of the cryohydrate to be  $-7^{\circ}\cdot 5$  to  $-8^{\circ}$ . But it is seen that as derived from a dilute solution I now find the cryohydrate to contain only 21·83 per cent. of  $\text{BaCl}_2$ , while in § 78, where it was derived from a saturated solution, I found 23·2. No doubt the smaller of these numbers is the more correct. The composition of the cryohydrate was derived from the following data. 6·5151 of melted cryohydrate gave 1·5225. The water-worth is accordingly 41·357. The solubility at  $0^{\circ}$  C. was derived from the following :—

	1.	2.	Mean.
Solution . . .	6·7830	7·9470	
$\text{BaCl}_2$ . . .	1·6214	1·9119	
Per cent. . . .	23·90	24·06	23·98

§ 149. *Separation of Ice &c. from Solutions of Protosulphate of Iron.*

TABLE XXX.

$\text{FeSO}_4$ per cent. by weight.	$\text{H}_2\text{O}$ per cent. by weight.	Temperature at which solidi- fication begins.	Nature of solid.
5	95	$-0\cdot 2$	Ice.
10	90	$-0\cdot 8$	"
14·5	85·5	$-2\cdot 0$	Cryohydrate.
14·9	85·1	0·0	$\text{FeSO}_4 + 7\text{H}_2\text{O}$ .

As in § 148, I find the composition of the cryohydrate to show somewhat less salt than previously (§ 82), where the cryohydrate was obtained from a saturated solution, and where the amount of salt was determined by simple evaporation. I now find the melting-point of the cryohydrate to be  $-2^{\circ}$  C.; and the temperature of the cryogen is exactly the same. In the above-given determinations of the cryohydrate and of the solubility at  $0^{\circ}$  C. the solution was oxidized with nitric acid, the sesquioxide was precipitated with ammonia and weighed; the amount of  $\text{FeSO}_4$  was thence deduced. For the cryohydrate,

4·8076 grms. gave  $0\cdot 3676 \text{ Fe}_2\text{O}_3$ , or  $0\cdot 6984 \text{ Fe SO}_4$ .

The water-worth is therefore 49·8. For the solubility at  $0^{\circ}$  C.,

7·7890 grms. gave  $0\cdot 6080 \text{ Fe}_2\text{O}_3$ , or  $1\cdot 1552 \text{ Fe SO}_4$ .

§ 150. *Separation of Ice &c. from Solutions of Tribasic Phosphate of Sodium and Hydrogen,  $\text{Na}_2\text{H PO}_4 + 2\text{H}_2\text{O}$ .*—This, the most usual phosphate of sodium, when used as a cryogen only reduces the temperature to  $-1^{\circ}\cdot 0$  C. The cryohy-



drate also melts at the same temperature (I found  $-0^{\circ}9$ ). Owing to this short range of temperature it is impossible to examine the separation of ice from solutions of various strengths. The determinations were made by reducing the salt to a bibasic pyrophosphate of sodium. For the cryohydrate,

	1.	2.	Mean per cent.
Solution .	6.5760	9.3401	1.72 of $\text{Na}_4\text{P}_2\text{O}_7$ , or
$\text{Na}_4\text{P}_2\text{O}_7$ .	0.1119	0.1627	1.83 of $\text{Na}_2\text{HPO}_4$ .

The water-worth is accordingly 852.

The following gave the solubility at  $0^{\circ}\text{C}$ . :—

6.7277 grms. of solution gave 0.1264 of  $\text{Na}_4\text{P}_2\text{O}_7$ , or 2.0 per cent. of  $\text{Na}_2\text{HPO}_4$ .

§ 151. *Separation of Ice &c. from Solutions of Nitrate of Sodium.*

TABLE XXXI.

$\text{NaNO}_3$ per cent. by weight.	$\text{H}_2\text{O}$ per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	$-2$	Ice.
10	90	$-4.2$	"
15	85	$-6.3$	"
20	80	$-8.4$	"
25	75	$-10.8$	"
30	70	$-13$	"
35	65	$-15.5$	"
40.80	59.2	$-17.5$	Cryohydrate.
42.34	57.66	0	$\left\{ \begin{array}{l} \text{NaNO}_3 \text{ or sub-} \\ \text{cryohydrate.} \end{array} \right.$

There appears to be a subcryohydrate of a composition intermediate between 40.80 and 42.34 per cent. For the determination of the composition of the cryohydrate I refer to § 75. The solubility at  $0^{\circ}\text{C}$ . was derived from the data; 6.0660 of solution gave 2.5682 of  $\text{NaNO}_3$ . It is remarkable that Marx found 44.4 per cent. and Poggiale 44.3 per cent. of the anhydrous salt. The salt I employed was made from the pure bicarbonate, and was entirely free from sulphates and chlorides. As in all similar determinations, I was not content to surround the flask containing the nitrate with ice; for I never find that by this means the temperature in the flask sinks to  $0^{\circ}\text{C}$ . A few grains of salt are added to the ice; and the temperature, as indicated by a continually agitated thermometer in the solution, remains for four or five hours at  $0^{\circ}\text{C}$ .

§ 152. *Separation of Ice &c. from Solutions of Nitrate of Lead.*

TABLE XXXII.

Pb 2(NO <sub>3</sub> ) per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	—0·3	Ice.
10	90	—0·5	"
15	85	—0·7	"
20	80	—1·2	"
26·23	73·77	—2·5	Cryohydrate.
29·89	70·11	0·0	Pb 2(NO <sub>3</sub> ).

The temperature of the cryogen is  $-2^{\circ}5$ . The solutions were analyzed by simple evaporation and gentle heating.

§ 153. *Separation of Ice &c. from Solutions of Nitrate of Barium.*—As the solution of this salt was found to contain only 5·37 per cent. of the anhydrous salt at  $0^{\circ}$  C., and as the cryohydrate only contains 5·3 (§ 80), only one intermediate observation on the separation of ice could be got, namely that of the 3-per-cent. solution, which gave up ice at  $-0^{\circ}4$ .

§ 154. *Separation of Ice &c. from Solutions of Nitrate of Strontium.*

TABLE XXXIII.

Sn 2(NO <sub>3</sub> ) per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	—0·5	Ice.
10	90	—1·2	"
15	85	—2·3	"
20	80	—3·8	"
26	74	—6	Cryohydrate.
29·62	70·38	0·0	Sn 2(NO <sub>3</sub> ).

§ 155. *Separation of Ice &c. from Solutions of Nitrate of Calcium.*

TABLE XXXIV.

Ca 2(NO <sub>3</sub> ) per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	—1·1	Ice.
10	90	—2·3	"
15	85	—4·2	"
20	80	—6·5	"
25	75	—9·3	"
30	70	—12·9	"

I have not yet got the cryohydrate of nitrate of calcium. As a cryogen it gives a temperature of  $-16^{\circ}$ .

§ 156. *Separation of Ice &c. from Solutions of Acetate of Sodium.*

TABLE XXXV.

Na C <sub>2</sub> H <sub>3</sub> O <sub>2</sub> per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	- 2.2	Ice.
10	90	- 5.1	"
15	85	- 9.1	"
20	80	-14.0	"
22	78	-16.0	"
23.3	76.7	-18.0	Cryohydrate.
26.6	73.4	0.0	Na C <sub>2</sub> H <sub>3</sub> O <sub>2</sub> + ? H <sub>2</sub> O.

The temperature of the cryogen is exactly  $-18^{\circ}$ . For the cryohydrate determination, 7.7032 grms. gave 1.7963 grm. of NaC<sub>2</sub> H<sub>3</sub> O<sub>2</sub>. For the solubility at  $0^{\circ}$ , 4.7473 grms. gave 1.2646 of NaC<sub>2</sub> H<sub>3</sub> O<sub>2</sub>. The water-worth is 30. The solid constituent was estimated by simple evaporation and heating to incipient fusion.

§ 157. *Separation of Ice &c. from Solutions of Acetate of Potassium.*

TABLE XXXVI.

K C <sub>2</sub> H <sub>3</sub> O <sub>2</sub> per cent. by weight.	H <sub>2</sub> O per cent. by weight.	Temperature at which solidification begins.	Nature of solid.
5	95	- 2	Ice.
10	90	- 4.6	"
15	85	- 7.4	"
20	80	-11.2	"
25	75	-16	"
30	70	-22.5	"

The solubility at  $0^{\circ}$  C. was measured both by directly weighing the dried and fused acetate, and by converting it into a nitrate by frequent treatment with nitric acid and evaporation.

1.	2.
Direct.	Indirect.
6.8131 solution gave	7.4837 solution gave
4.2083 K C <sub>2</sub> H <sub>3</sub> O <sub>2</sub> , or	4.8573 K NO <sub>3</sub> , or
61.8 per cent.	62.8 per cent. K C <sub>2</sub> H <sub>3</sub> O <sub>2</sub> .
Mean . . . .	62.3.

The cryohydrate of acetate of potassium has not yet been determined.

The temperature of the cryogen of acetate of potassium is  $-25^{\circ}$  C.

It is well known that fused acetate of potassium is heavier than the anhydrous solid acetate at the same temperature; but I do not find the remarkable fact anywhere noticed, that the fused anhydrous salt is also lighter than the aqueous solution of the salt saturated at ordinary temperatures ( $17^{\circ}$  C.).

(2) *A few Organic Crystalloids in Aqueous Solution; their behaviour on being cooled and on being heated, and as Cryogens.*

§ 158. *Cane-sugar.*—Sugar-candy was finely powdered and dried at  $100^{\circ}$  C. in a current of dry air.

TABLE XXXVII.

$C_{12}H_{22}O_{11}$ per cent. by weight.	$H_2O$ per cent. by weight.	Temperature at which solidification begins.	Nature of solid formed.
5	95	$-0.3$	Ice.
10	90	$-0.5$	"
15	85	$-0.9$	"
20	80	$-1.3$	"
25	75	$-1.8$	"
30	70	$-2.4$	"
35	65	$-3.2$	"
40	60	$-4.1$	"
45	55	$-5.4$	"
50	50	$-7.0$	"
51.4	48.6	$-8.5$	Cryohydrate.
(Mean) 67.33	32.67	0.0	$C_{12}H_{22}O_{11}$ .

Solutions stronger than the cryohydrate may be cooled below  $-8^{\circ}.5$  before solidification begins. A 60-per-cent. solution does not show signs of solidification above  $-11^{\circ}.4$ . The solid which separates, however, is sugar. This is a consequence of the supersaturability of sugar solutions, and the possibility of the condition of supersaturation being maintained even in solutions from which crystals of sugar are being deposited. Also, as was so notably the case with alcohol, the cryohydrate is an abundant solvent for sugar. The cryohydrate of saccharose was analyzed by evaporation.

2.2658 grms. gave 1.5244 gm. of saccharose, or 51.4.

For the solubility at  $0^{\circ}$  a solution saturated at  $+20^{\circ}$  was kept for three days and nights at  $0^{\circ}$  C.

$\left. \begin{array}{l} 3.6300 \\ 4.1175 \end{array} \right\}$  gave  $\left. \begin{array}{l} 2.4510 \\ 2.7640 \end{array} \right\}$  of saccharose or cane-sugar.

Since it will be shown in § 161 *et seq.* that the colloid forms of matter not only do not unite with water, but also do not depress the freezing-point of that liquid, it did not seem unlikely that on cooling below  $0^{\circ}$  a solution of raw sugar containing about 50 per cent. of solid matter, the solid cryohydrate would be formed and the sugar be freed from colloid impurities. As caramel certainly, and probably other foreign colouring-matters in raw sugar are colloids, I have succeeded in greatly purifying raw sugar by this method of cryohydration. Into the details of this I shall not here enter.

§ 159. *Glycerine*.—With regard to this substance a very remarkable circumstance may be noticed. That it is crystalloid we have had until lately (1) the indirect evidence depending upon its being an alcohol, and upon several alcohols being known in the solid and crystalline state, while others which are not so known yet unite with crystalline salts; (2) the direct evidence obtained from its diffusion through colloid septa. Lately it has been observed to assume the form of a crystalline solid. Again, it has lately been employed in aqueous solution in Pictet's ice-machine as a non-freezable liquid to yield heat to vaporizing sulphurous acid, and take it from water for the purpose of freezing the latter. The latter faculty of its solution to resist solidification below  $0^{\circ}$  C. proves, first, that it will form a cryogen, and, secondly, that it will form a cryohydrate—the latter fact again proving, as we shall see in § 161 *et seq.*, that it is a crystalloid.

Pure glycerine dried by being kept for a week over oil of vitriol *in vacuo*, when mixed with finely crushed ice forms a cryogen whose temperature is  $-19^{\circ}$  C.

TABLE XXXVIII.

$C_3H_5O_3$ per cent. by weight.	$H_2O$ per cent. by weight.	Temperature at which solidification begins.	Nature of solid formed.
5	95	$-0.8$	Ice.
10	90	$-2.0$	"
15	85	$-3.3$	"
20	80	$-5.0$	"
25	75	$-6.2$	"
30	70	$-8.8$	"
35	65	$-11.5$	"
40	50	$-13.9$	"
45	55	$-16.7$	"

I have not yet succeeded in obtaining the cryohydrate of glycerine.

As a cryogen the glycerine behaves as hydrochloric acid and other liquid elements of cryogens; namely, the temperature obtained is lower if the liquid be previously cooled.

§ 160. *Tartaric Acid*.—As a cryogen, tartaric acid gives the temperature  $-13^{\circ}5$ . I cannot say that I have cleared up all the ambiguities in the behaviour of this body. The separation of ice took place with regularity as far as the following solutions are concerned.

$C_4H_6O_6$ per cen by weight.	$H_2O$ per cent. by weight.	Temperature at which ice sepa- rates.
5	95	$-0.7$
10	90	$-1.4$
15	85	$-2.5$
20	80	$-3.7$
25	75	$-4.7$
30	70	$-6.3$
35	65	$-7.6$
40	60	$-10.1$
45	55	$-13$

Apparently continuous with these results is the separation of a solid at  $-16^{\circ}5$  from a 50-per-cent. and of a solid at  $-17^{\circ}6$  from a 55-per-cent. solution. But there is here really a breach of continuity. The 55-per-cent. gives up tartaric acid at  $-17^{\circ}$  when crystals of that body are introduced. And it is difficult to say whether ice, or acid, or a cryohydrate is separated from the 50-per-cent. at  $-16^{\circ}5$ . If a 50-per-cent. solution be cooled to  $-19^{\circ}$  and a smaller portion of the same solution which has been solidified by an ether-carbonic-acid cryogen be introduced into the first, the temperature rises to  $-8^{\circ}$ , and opaque crystals are formed, while that temperature is constantly maintained. The same body at the same temperature is formed even when the first solution has only been cooled to  $-14^{\circ}$ . I think, therefore, that there is with tartaric acid a body similar to that which we encountered with the iodide of sodium, namely a subcryohydrate. Owing to the sirupy nature of these strong solutions at these low temperatures, and the very slow rate at which the separation of their constituents ensues, I do not think that analyses of such intermediate subcryohydrates would be sufficiently trustworthy for reproduction. But the cryohydrate which separates at  $-8^{\circ}$  is distinct enough.

4.0735 grms. of solution gave 1.8972 gram. of  $C_4H_6O_6$ , or 46.6 per cent.

A solution kept for two days and nights at  $0^{\circ}$  showed the

following composition: 5.1304 grms. of solution gave 2.7233 grms. of  $C_4H_6O_4$ , or 53.0 per cent.

(3) *Colloids in aqueous solution; their behaviour on being cooled and on being heated.*

§ 161. *Generalities.*—It is perhaps only by an extension of the definition that we can regard solutions of colloids as solutions of salts. It will be of great interest to examine how far the two classes of bodies resemble, and how far they differ from one another in aqueous solution. The substantial researches of Graham on the division of matter into crystalloids and colloids, resulting as they did in the discovery of a whole series of new and profoundly interesting bodies, threw a flood of light on molecular physics. He showed that colloid septa were permeable only to crystalloid liquids, and thus established among the molecular mechanism of matter a series of connected facts similar to and of no less importance than those which have of late attracted so much attention in the domain of radiation; for that sonorous undulations are absorbed by sympathetic vibrators, that radiation is arrested by the possible authors of such radiation, cannot long be separated from the fact that colloids arrest colloids by contact-continuity, while they are permeable (transparent, diathermanous) to crystalloids.

Recalling a few of the distinguishing properties of colloids, we find that when miscible at all with water they are miscible in all proportions. Accordingly one colloid cannot precipitate another from solution. Towards water they then behave as two gases behave towards one another. Some gelatinize (gelatine); others do not (gum-arabic). Some coagulate by heat (albumen) in association with water. In the dry state they are amorphous and show conchoidal fracture. I look upon a water-jelly as consisting of an indefinitely minute cellulation of a solid colloid enclosing liquid water. In a liquid water-colloid the solid colloid particles are present, but they do not form closed cells, and therefore do not with their contents resist change of form; nor are they necessarily in contact with one another. The viscosity of such a mass is due to friction between its solid and liquid parts. The elasticity of a jelly is due to the elasticity of its cell-walls. Thus I can form a model of a jelly by pressing together little caoutchouc bladders containing water: and in soap-suds I recognize a jelly of a higher order; that is, instead of solid we have liquid cell-walls, and instead of liquid we have gaseous contents. On heating or stirring the suds the bubbles burst, the liquid walls collapse and contract, not again to be formed automatically. On heating or stirring a jelly the

solid colloid walls are broken, the liquid contents of the individual cells become continuous, so that all hydrostatic laws hold good. Let the heated liquid cool, and similar cells are restored; but on repeated heating, the cells are burst in such divers places that they are not to be restored. Such restoration is also hindered and prevented on stirring during cooling.

I gladly leave these speculations for the facts which gave them birth. Through the kindness of Mr. W. C. Roberts, F.R.S., there were placed at my disposal certain of the inorganic colloids prepared by the late Mr. T. Graham, the Master of the Mint. Some of these are spontaneously undergoing so interesting a change that I have been unwilling to disturb them. Others, notably a solution of silicic acid and of sesquioxide of iron in water, I find contain each under 1 per cent. of oxide. I prepared, therefore, fresh solutions, following the directions given by Mr. Graham, and meanwhile examined certain organic colloids.

§ 162. *Gum arabic*.—If a 1-, 5-, 10-, 20-, 25- or 30-per-cent. aqueous solution of gum arabic be cooled, ice begins to be formed in all cases at  $0^{\circ}$  C. exactly. Even a 35-per-cent. solution, which is very viscid, begins to give up ice at  $0^{\circ}$ , and is quite solid at  $-0^{\circ}\cdot5$ . A 45-per-cent. solution, which is semisolid, shows a temperature of  $-1^{\circ}$  at the beginning of sensible ice-formation, and is certainly solid at  $-1^{\circ}\cdot2$ . This very small degree below  $0^{\circ}$  C. in so viscid a substance is certainly not more than may be attributed to the overcooling of the ice already formed. I can find no trace of a cryohydrate; and that such does not exist is supported by the fact that, in spite of the great solubility of gum in water, when powdered gum and ice are mixed together the resulting temperature is  $0^{\circ}$  exactly. Gum arabic is therefore powerless as a cryogen. It is true that this might be attributed to the gum acting like a certain quantity of anhydrous chloride of calcium, which when mixed with ice may compensate, by the liberation of heat of combination, for the heat absorbed in the joint liquefaction. But that such balancing does not ensue is shown by the fact that a 45-per-cent. solution of gum also gives  $0^{\circ}$  C. with ice, while a solution, however weak, of chloride of calcium will absorb heat when liquefying ice by contact. The same is also proved by the observation that when gum is mixed with water the alteration in temperature, though certainly in the direction of heat-liberation, is very inconsiderable. In a preliminary experiment I found that powdered gum and water mixed in indefinite proportions gave a rise of  $0^{\circ}\cdot75$  C. As the result of a number of more exact experiments, it ap-



peared that the greatest heat was developed when four parts by weight of water were mixed with one of finely powdered gum arabic; a rise of  $1^{\circ}1$  ensued, namely from  $15^{\circ}7$  to  $16^{\circ}8$ . The first temperature was that of the water in the outer of two concentric beakers, the inner of which contained the gum. The beakers were thickly wrapped and covered with flannel, and had been in such contact for several hours. To the above numbers, however, I do not attach much quantitative value; for, owing to viscosity, solution takes place very slowly. The results, however, clearly show that the heat developed is far too small to counteract any great heat-absorption by solution, and that accordingly the latter does not exist. The heat liberated is indeed scarcely more than the heat of capillarity.

I believe that most of us would have been disposed to predict that in order to boil a solution of gum a temperature above  $100^{\circ}$  C. would be required. This was, I confess, my own persuasion until I was enlightened by the above experiments on the separation of water as ice from gum-solutions of all strengths. But being so enlightened I was able to predict that a solution of gum would boil at a temperature not above  $100^{\circ}$  C. A 20-per-cent. solution of gum arabic appeared to boil in a test-tube over the bare flame below  $100^{\circ}$  C. But as it is almost impossible to avoid a certain amount of decomposition under these circumstances, a tube holding a fresh quantity of the 20-per-cent. solution was placed in a wider tube containing a solution of chloride of sodium which boiled at  $104^{\circ}$  C. On heating the latter gradually the gum-solution could be made to boil before the brine began to do so. And if by rapid heating the latter were made to boil, the gum-solution boiled violently, and continued to do so after the brine had ceased. A thermometer in the gum-solution showed  $100^{\circ}$  C. Further, a 40-per-cent. solution of gum arabic when surrounded by boiling water boils at  $98^{\circ}$  C., or two degrees lower than the water.

§ 163. *Albumen*.—Dry albumen when mixed with ice can only depress the temperature  $0^{\circ}25$  C. Four parts by weight of water and one of albumen, both at  $18^{\circ}2$ , gave, on mixture, a temperature of  $19^{\circ}5$ . In another experiment the common temperature rose from  $19^{\circ}$  to  $21^{\circ}$ . In the above experiments commercial albumen was used. For more exact experiments white of egg was used, which analysis showed consisted of 13.37 per cent. of albumen (dried at  $100^{\circ}$  C.) and 86.63 of water. Such white of egg begins to separate ice at  $0^{\circ}$  C., and freezes into a solid mass at  $-0^{\circ}5$ . A solution diluted to 5 per cent. begins to give up ice at  $0^{\circ}$  C., and is solid at

$-0^{\circ}2$  C. A 10-per-cent. solution separates ice at  $0^{\circ}$  C., and forms a solid mass at  $-0^{\circ}3$  C. The boiling-points of albumenoid solutions, of course, could not be determined. And the addition of alkalies to prevent coagulation would naturally vitiate the results.

§ 164. *Gelatine*.—The gelatine used in the following experiments was such as is commonly used by confectioners. Solutions of this substance containing 5, 10, 15 and 20 per cent. may be rapidly cooled under constant stirring without gelatinization. Ice in all such cases begins to be formed at  $0^{\circ}$  C.; and the whole becomes solid at that temperature. Stronger solutions are so difficult to manipulate, on account of their gelatinization and consequent hardness, that the results with them are doubtful. As far as the experiments extend, however, they fail to show that the freezing-temperature is affected.

A 20-per-cent. solution heated (as in § 162) in a brine-bath boiled at  $99^{\circ}9$  C. A 45-per-cent. solution boiled at  $97^{\circ}5$ ; and a 50-per-cent. solution, which is as stiff as the strongest glue used by cabinet makers, boiled also at  $97^{\circ}5$ . A very striking and conclusive experiment is, to surround the tube containing the 50-per-cent. solution with distilled water in another tube. If the exterior water-tube be heated slowly, the glue in the inner tube boils before the water in the outer one. On continually stirring the frothy viscid mass, the true boiling-point is observed to be constant at  $97^{\circ}5$ .

As an experiment the above is exceedingly instructive, and, I believe, novel; but, like so many novel and instructive phenomena, it is one of daily occurrence. The blistering and frothing of the strongest glue when heated by steam or water in the outer pot has certainly been observed hundreds of times in every carpenter's shop. Is it strange or is it natural that so familiar a fact should never have struck us as requiring explanation?

§ 165. *Mixtures of organic colloids*.—To round off the series of experiments with colloids homologous with those of crystalloids, I next examined a mixture of gum arabic and gelatine. A solution was made containing 20 per cent. of gelatine, 20 of gum arabic, and 60 of water. This began to freeze and froze completely at  $0^{\circ}$  C. It boiled at  $97^{\circ}7$  C.

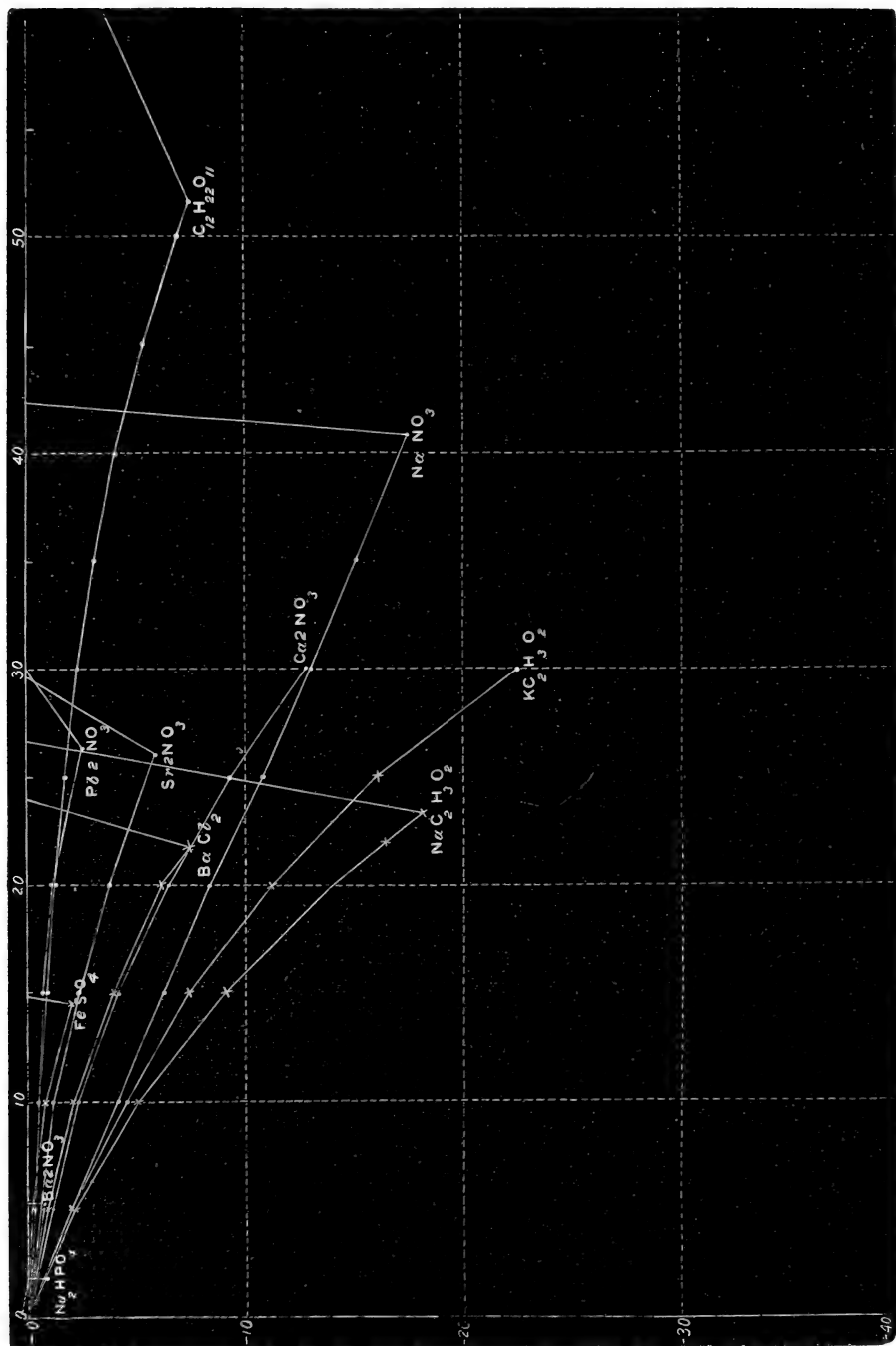
#### (4) *General remarks on colloids*.

§ 166. The depression of the boiling-point of water by the solution in it of organic colloids has its analogies. Amongst the metals innumerable instances occur to us in which the melting-point of an alloy is not only lower than that of one constituent,

not only lower than the mean of the two, but lower than that of either or any of the constituents—witness the “fusible metals.” When metals are alike in chemical affinities, they have little affinity for one another. They mix mainly by diffusion. They dilute one another. If we mix sodium with sodium, we certainly dilute each sodium with the other; the adhesion which we supply is exactly equal to the cohesion which we replace; the resulting mass is identical with its constituents. But if we mix potassium and sodium together, by diffusion we diminish cohesion, and the adhesion introduced is less than sufficient to replace it; from two solids we get a liquid. The similarity of the metals prevents their having any notable relationship with one another. Equally to the purpose is the recollection of the alteration in boiling-point which follows the mixture of liquids which are chemically neutral to one another. Many a month has been wasted in the attempt to get propylic alcohol from fusel oil, owing to the fact that a mixture of amylic and butylic alcohols will commence to boil at a temperature below the boiling-point of butylic alcohol. Moist ether boils below dry ether; and even a liquid such as iodide of ethyl, which is almost insoluble in water and which dissolves but little of that body, has its initial boiling-point already lower than that of water. The work done by the heat in separating the particles of a body is already effected by the interpenetration of the associated body. Cohesion is diminished and a less heat-tension is required to complete the separation of the more volatile constituent as vapour.

From another aspect we may consider the neutral body to act as an infinite series of points, and recall the fact that, on evaporating saturated solutions of various salts over a steam-bath, bubbles are frequently formed beneath the salt-pellicle, owing perhaps to the multiplicity of points, at temperatures certainly below  $100^{\circ}\text{C.}$ , while the clear solution of the same salt when saturated demands a temperature several degrees above  $100^{\circ}\text{C.}$  for ebullition. The rise in temperature shown on mixing a dry colloid with water is probably connected with the deficit below  $100^{\circ}\text{C.}$  of the temperature required to boil the resulting colloid solution.

Returning to the separation of ice from colloid solutions, we must conceive that when ice so separates that the solution becomes enriched in regard to the colloid body, the particles of ice in contact with the source of cold become overcooled (that is, fall below  $0^{\circ}\text{C.}$ ) on account of the insufficient circulation and the imperfect thermal conductivity of ice and of the colloid solution. But when such over-cooled ice is stirred with the colloid solution a fresh portion of ice is formed if the latter is already at  $0^{\circ}\text{C.}$ ; if above  $0^{\circ}\text{C.}$ , then some of the



ice melts : in either case the  $0^{\circ}$  C. is restored. Ultimately the solid particles of the anhydrous colloid must lie side by side with the ice particles, as they do when the two are mixed as powders, as in § 162. They are as independent of one another as are the particles of ether and chloroform when the two are mixed, or as are the particles of two gases which diffuse through one another without combination.

I reserve the results which I have obtained with inorganic colloids for a future communication.

The figure shows the same constants as were shown in the figure, Paper IV., but with the new salts.

XXIX. *Notices respecting New Books.*

*The Moon, and the Condition and Configurations of its Surface.* By EDMUND NEISON, F.R.A.S. &c. London: Longmans, Green, & Co.

MANY distinct branches of observational astronomy have been cultivated of late years, some to a much greater extent than others ; that which has received the smallest amount of attention is Selenography, especially the topography of our satellite or the configurations of its surface. It is true a few energetic workers have been engaged for some years past in studying certain portions of the lunar disk ; still the progress of selenography has been slow as compared with that of other departments of astronomy. The work now before us, if we are not greatly mistaken, will have an opposite influence on the progress of the study of the moon's surface to that which Mädler's great work produced in his day ; for, the opinion which prevailed (alluded to by our author) being that the moon was an airless, waterless, lifeless, unchangeable desert, astronomers then directed their attention to other fields of research, and selenography rested on its laurels, making no further progress for many years.

The author has divided his work into two portions, the first of which consists of five chapters, in which he treats (1) of the motions, figure, and dimensions of the moon, (2) of the physical condition of its surface, (3) of the lunar formations, (4) of lunar history, and (5) of the variations of the surface. The greatest portion of the materials of Chapter I. may be found in most astronomical works ; the second chapter, however, treats at some length on the much-disputed question of a lunar atmosphere, the author considering that its existence must be regarded as certain. The characteristics of the lunar surface, especially in connexion with terrestrial analogies, are well treated in this chapter, as well as its temperature as modified by the presence of an atmosphere. The concluding paragraph of this chapter is important as bearing on the analogy of lunar as compared with terrestrial forces. "The gradual progress in our knowledge of the present condition of the surface of our satellite is surely, if slowly, pointing out in unmistakable terms the entire analogy in nature, if not wholly in degree, of the forces that have moulded the surfaces of the earth and moon to their present state."

In the third chapter the author divides the entire surface of the moon into three great classes—namely plains, craters, and mountains, the former consisting of the dark and light plains. The well-known formations, “craters,” he divides into nine classes—namely walled plains, mountain rings, ring plains, crater plains, craters, craterlets, crater pits, crater cones, and depressions; while he reckons as many as twelve classes of mountains—namely the great ranges, highlands, mountains, and peaks, constituting the greater elevations, while the hill-lands, plateaus, hills, and mountain-ridges form the lesser; he also particularizes hillocks, mounds, ridges, and land-swells as comprising the smaller irregularities of the surface. Although the number of these divisions is somewhat extensive, it will be useful in studying the smaller lunar features, especially such as occur on the walls of the larger plains. Portions of these walls Mr. Neison is disposed to regard as forming parts of long continuous mountain-ranges—a view, if we mistake not, which has been previously promulgated—many of the walled and ring-plains “appearing,” he says, “more like confluent plains or spaces enclosed by intersecting mountain-chains.” The most interesting objects in the author’s classification of craters are “crater-cones,” of which he says:—“Perhaps the true representatives upon the moon of the existing terrestrial volcanoes have only of late years had their distinctive features recognized, and even when exceptionally well placed are only with difficulty distinguishable from the bright mountain-peaks and minute white spots dispersed over the lunar surface. Steep conical hills or mountain-peaks, from one half to two or three miles in diameter, with deep cone-like central depressions scarcely one half of this size in diameter, they remain in high illumination, visible as white brilliant spots of minute dimensions, and only under favourable conditions can their central apertures be detected even in the most powerful instruments. . . . Under any circumstances they are difficult to distinguish from bright mountain-peaks and white spots, whilst in many ways they bear a considerable resemblance to terrestrial volcanic cones; and it is therefore not without interest to find that many phenomena perhaps indicating actual lunar changes present themselves in observing these objects.” In this chapter the author draws attention to those remarkable features known as rills or clefts, and the great ray-systems, also to the different varieties of brightness and colour found on the surface of our satellite.

Chapter IV. is devoted to general lunar history; we, however, think that, had the author given separately the two branches of history, viz. that which culminated in the advanced state of the lunar theory and that which has reference to the surface, both (as at present) arranged chronologically, some little trouble to the selenographer would have been saved.

The last chapter, V., of those forming the Introduction treats of the variations of the surface; and of these the author has taken, as it appears to us, the most consistent and truthful views, considering that so little is really known of the vast majority of the minor details of the moon’s surface. He says that “a very considerable alteration of the present constitution of the surface might occur at

any moment, without the slightest probability of its being detected as an instance of physical change on the moon. Thus, for example, if on the moon proportionately the same amount of volcanic energy were every year manifested in a similar manner to what occurs on the earth, there is no reason whatever for supposing that it would have been hitherto detected. With the present condition of our acquaintance with the topography of the lunar surface, in this state, it is not in the slightest degree surprising that no definite instance of volcanic energy on the moon has hitherto unmistakably declared itself. Nor can this circumstance justify its being held that selenological volcanic activity must have long ere this entirely ceased to exist." As one of the instances in which there may be great probability of physical change having taken place, Mr. Neison gives the well-known case of Linné, which he treats with considerable impartiality, and shows that, so far as the evidence collected in 1867 and 1868 bears on the question, it is the difficulty of reconciling Schröter's with Lohrmann's and Mädler's drawings which renders any conclusion as to a real change having taken place unsatisfactory. For his treatment of this vexed question we must refer the reader to the work itself, pp. 185-192. We may, however, remark that the whole matter turns upon the identification of one or other of two of Schröter's spots with Linné; on this point the author says:—"Birt's view of the identity of *g* and Linné, and of *v* with the small white spot known to exist south of it, merits consideration; for if this be correct, the weight laid upon Schröter's drawings as showing that in his time Linné presented the same appearance as now, rests on a misconception; and with the correction of this the entire fabric constructed to demonstrate the absence of any change in Linné crumbles into ruin."

The second and by far the largest portion of the work consists of a description of the 513 named objects on the surface of the moon, accompanied by twenty-two maps showing their localities. This portion, there can be no doubt, must be found of great value to the selenographer. It would, however, have contributed to easy reference had each formation been preceded by a number, as in Webb's book. The information contained in the description of these objects supplies a want increasingly felt by every student of the moon's surface. A chapter is added on selenographical formulæ, also tables of points of the first order, lunar elements, &c.

Taking a general view of the work, it is one that brings before the student nearly the whole of the known desiderata of selenography, which, combined with the valuable information it contains, renders it a most important acquisition; we notice, however, the absence of one characteristic which might have been introduced with advantage. Mr. Neison repeatedly urges the necessity of certain features being subjected to a close and searching investigation; a short chapter on the best method of conducting such investigations would greatly contribute to guide observers who may be just entering on the study of selenography; and no one could pen instructions of the kind so well as the author, whose success both as regards this work and his future investigations we most sincerely desire.

*Report on the Superinduced Structure of Rocks, called Jointing; and its relation to Slaty Cleavage.* By Prof. W. KING, Sc.D. &c. (Trans. Royal Irish Acad. vol. xxv. pp. 605-662, with plates xxxiv. to xxxviii.) 4to. 1875.

Joint-structure of rocks has necessarily commanded the attention of geologists and others who deal with rock-masses, whether practically or theoretically, because it has to do with the mechanical treatment of stone in quarries, and with the history of the rocks (as a superinduced structure). The jointing of basalt, granite, coal, and of various limestones and sandstones, the lamination of schists, and the cleavage of slates have had, and still receive, much attention from surveyors and physicists, many of whom are mentioned in the first paragraph of Professor W. King's memoir. The Royal Irish Academy has furthered the author's researches in this subject; and his views are expressed and illustrated in this memoir under two headings:—I. Jointing, its character, its extensions, its original position, its dislocation, its development at different periods, its origin; and II. Its relation to slaty cleavage.

1. Varieties of jointing and cleat, close or open, fine or rough, irregular or geometrical, &c. are described. 2. The apparently more or less meridional or equatorial directions and extension of joints, and their original perpendicularity are insisted upon. 3. The present oblique position of joints or coarse cleavage in some strata lying between non-jointed beds is explained by the lateral shift of such perpendicularly jointed strata. 4. The occurrence of two or more sets of jointings at different angles, in successive groups of strata, according to the successive jointing and occasional shifting of some of the strata, illustrates the development of this structure during different periods, and accounts for the predominance of some sets of joints and veins over others. 5. The origin of joints is referred to as a physical phenomenon due to "divisional forces akin to those which give rise to mineral cleavage;" and, lastly, slaty cleavage is said to be "essentially the result of pressure exerted against divisional planes, chiefly belonging to jointing, that existed in any given rock prior to its becoming affected by such pressure."

*Fundamental Principles of Arctic Investigation.* By Lieut. C. WEYPRECHT, I. R. Austrian Navy. In an Address to the Meeting of German Naturalists at Gratz, Sept. 18, 1875.

Lieutenant Weyprecht points out the immense advantages that would accrue to science and general well-being from a perfect knowledge of Terrestrial Magnetism and Meteorology, the real basis for which is only to be found in regulated, continuous, and synchronous observational study in circumpolar regions, and, for the northern hemisphere, necessarily within the arctic circle. He fully appreciates the value of polar investigation in astronomy and geodesy; also the importance of the polar flora and fauna, and the study of life, whether migratory or fixed, under such extreme conditions; also geological phenomena and their bearings, and the interest felt in geographical discovery. He regrets, however, that the race for the pole



is made the primary object of expensive expeditions. The establishment of more observatories in arctic lands, and the strengthening of those already formed, would be of far greater service to science, and productive of greater gain to navigation and general useful knowledge. Austria is willing to join in international establishments of this kind. He refers to Sir J. C. Ross's Antarctic Expedition as a model scientific voyage, and draws attention to the good results of the economic expeditions wintering in the Spitzbergen archipelago, and devoted to physical and meteorological observation rather than to geographical discovery. The loss of the open weather by long sledging expeditions destroys real scientific research. Arctic research is of the highest importance to the knowledge of nature's laws ; but independent series of observations (especially if chiefly devoted to geography) have but little value, compared with stationary observatories encircling the arctic lands, and working on synchronous magnetic "term-days" and other agreed times and periods, without which physical, astronomical, and meteorological phenomena cannot be reduced to their natural laws.

### XXX. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 151.]

Mar. 2, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

**T**HE following paper was read :—

"Preliminary Note on the Compound Nature of the Line Spectra of Elementary Bodies." By J. N. Lockyer, F.R.S.

In a former communication to the Royal Society (Proc. vol. xxii. p. 380, 1874) I referred briefly to the possibility that the well-known line spectra of the elementary bodies might not result from the vibration of similar molecules ; and I was led to make the remark in consequence of the differences in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

I have now clear evidence that the molecular grouping of calcium which, with a small induction-coil and small jar, gives a spectrum with its chief line in the blue, is nearly broken up in the sun (and quite broken up in the discharge from a large coil and jar) into another or others with lines in the violet.

I say "another" or "others," because I have not yet been able to determine whether the last-named lines proceed from the same or different molecules ; and it is possible we may have to wait for photographs of the spectrum of the brighter stars before this point can be determined.

This result enables us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to the degree of dissociation at present at work in the sun.

I beg permission to append the following Letter from Prof. Stokes, and my reply :—

March 3, 1876.

MY DEAR LOCKYER,—You might perhaps like that I should put on paper the substance of the remarks I made last night as to the evidence of the dissociation of calcium.

When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases, not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus if A, B, C, D, E denote conspicuous bright lines, of increasing refrangibility, in the spectrum of the vapour, it might very well be that at a comparatively low temperature A should be the brightest and the most persistent; at a higher temperature, while all were brighter than before, the relative brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E. If, now, the quantity of persistence were in each case reduced till all lines but one disappeared, the outstanding line might be A at the lowest temperature, C at the higher, E at the highest. If so, in case the vapour showed its presence by absorption but not emission, it follows, from the correspondence between absorption and emission, that at one temperature the dark line which would be the most sensitive indication of the presence of the substance would be A, at another C, at a third E. Hence, while I regard the facts you mention as evidence of the high temperature of the sun, I do not regard them as *conclusive* evidence of the dissociation of the molecule of calcium.

Yours sincerely,

G. G. STOKES.

5 Alexandra Road, Finchley Road, N.W.,  
March 5, 1876.

DEAR PROFESSOR STOKES,—I was not prepared for your suggestion, as it was the abnormal and not the normal behaviour of Ca which led me to investigate it.

D is darker than any other of the Na lines, and H in the chromosphere at the Ca level is red, while in the coronal atmosphere it is green; *i. e.* the *least* refrangible line is developed by increase of temperature, and not the more refrangible one\*.

I am not the less grateful to you for your suggestion; and so soon as I can obtain the use of a more powerful coil I will go over the ground as completely as I can.

Are you quite sure that the molecular structure of the platinum wire is constant while it behaves as you say it does?

\* [The point, however, is, Which is the *most persistent* line at the respective temperatures, *i. e.* the last to disappear when the quantity of substance present is continually reduced? and Mr. Lockyer himself has shown that the line which is the most conspicuous when there is substance enough present to show several lines is by no means necessarily the most persistent.—G. G. S.]

I beg you will permit your letter and this to appear in the Proceedings. It will make my note more useful if you will.

Believe me

Very faithfully yours,

J. NORMAN LOCKYER.

March 23.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following paper was read:—

“On the Force caused by the Communication of Heat between a Surface and a Gas, and on a New Photometer.” By Prof. Osborne Reynolds.

This paper contains an account of an experimental investigation undertaken with a view to support, by absolute measurements, the theoretical arguments by which the author endeavoured to prove the existence of reactionary forces or “heat-reactions” whenever heat is communicated from a surface to a gas, and *vice versa*, and the connexion between these forces and the motion caused by heat and light falling on bodies *in vacuo*.

Since the publication of the author’s paper on this subject in the ‘Proceedings’ for April 1874, the correctness of his conclusions with regard to the existence of these heat-reactions has not been controverted or even questioned; while Professors Tait and Dewar, after an elaborate investigation, entirely confirm the author, not only in these conclusions, but also in his view as regards the explanation of Mr. Crookes’s experiments. Mr. Crookes, however, appears entirely to repudiate this explanation, arguing,—

1. That he obtains his best results in vacua so perfect that there is no air either to receive the heat or react on the surface.

2. That the force is *radiant* in character.

3. That light, as well as heat, produces the motion, which consequently cannot be due to the heating of the surface.

Having obtained one of the beautiful little “light-mills” constructed by Dr. Geissler, of Bonn, the author was in a position to make quantitative measurements of the effects produced and of the force producing them.

In the first place, with regard to the sufficiency of the residual air to cause the motion. It was found that this air is, with the exception of the friction of the pivot, which is found to be so small as to be inappreciable, the sole cause of the resistance which the mill experiences, of the limit which is imposed on its speed for each intensity of light, and of the rapidity with which it comes to rest when the light is removed. The law of resistance, as determined by careful measurements, is found to agree perfectly with the resistance which highly rarefied air would offer to its motion; and this law is distinctly special in its character, being proportional to the velocity at low speeds, and gradually tending towards the square of the velocity as the speed increases.

Having established the fact that there is sufficient air in the mill (and Mr. Crookes’s behaves in the same manner as this mill) to balance, by its resistance, the force which moves the mill, it is argued that all question as to the sufficiency of the air to cause the forces is removed. What the air can prevent it can cause.

As regards the possibility of the motion being in any way the direct result of radiation. This supposition the author had previously shown to be directly contradicted by the fundamental law of motion, that action and reaction are equal. A cold body runs away from a hot body, while, if free to move, the hot body will run after the cold body, showing that the force does not act from body to body, but that each body propels itself through the surrounding medium in a direction opposite to its hottest side, the effect of one body on the other being due solely to the disturbance which it causes in the equilibrium of temperature.

The truth of this view was entirely confirmed by an experiment made by Dr. Schuster, to be communicated to the Royal Society. Dr. Schuster, by suspending the entire mill, was able to see whether the force which causes the vanes of the mill to revolve caused any twisting force on the envelope; and he found that such twisting force, so far as it existed, was exactly what must result from a force arising entirely within the mill, *i. e.* between the vanes and the medium immediately surrounding them. While the vanes were acquiring momentum a reaction was experienced by the envelope; but when the vanes had acquired full speed, the envelope was subjected to no force whatever; when, however, the light was turned off, the vanes, by virtue of the friction they experienced, tended to drag the envelope with them.

Besides proving that the force acts between the vanes of the mill and the medium immediately surrounding them, Dr. Schuster's experiments furnish a quantitative measure of the actual force. Taking the manner of suspension and the weight of the mill into consideration, the effect produced showed that, when making 240 revolutions per minute, the torsional force on the vanes does not exceed one forty millionth part of a pound acting on a lever a foot long; that the pressure of the gas on the vanes to produce this was not more than one two million five hundred thousandth part of a pound on the square inch, or one thousandth part of the pressure in a Torricellian vacuum, thus placing the extreme minuteness of the forces in a clear light, a light from which the extreme delicacy of Mr. Crookes's instrument had altogether withdrawn them.

It is then shown, on theoretical grounds, that the difference of temperature on the two sides of the vanes necessary to cause heat-reactions of this magnitude could not be less than  $1^{\circ}7$  F., while the probability is that it is considerably more.

In order to apply this test and see how far the actual difference of temperature in Dr. Schuster's experiments corresponded with that deduced from the theory, a new photometer was devised by the author with an immediate view of measuring the difference of temperature caused by light on a black and a white surface.

Of two thin glass globes,  $2\frac{1}{2}$  inches in diameter, connected by a siphon-tube  $\frac{1}{8}$  inch internal diameter, one was blackened with lampblack on the inside over one hemisphere and the other was whitened with chalk in a similar manner, the two clean faces of the globes being turned in the same direction. Oil was put

in the tube, and the globes were otherwise sealed up. Any light which enters through the clean faces is received on the black and white surfaces; and the air in the globes expands in accordance with the difference of temperature which they attain, moving the oil in the tube. A motion of  $\frac{1}{2}$  an inch on the part of the oil shows a difference of  $2^{\circ}\cdot 2$  in the temperature of the air within the globes.

The instrument so constructed is exceedingly delicate, and will show a difference in the intensity of light sufficient to make one revolution per minute difference in the speed of the mill. As a photometer it is much more convenient than the mill; and its construction presents much less difficulty. By making the lower portion of the siphon-tube horizontal, and using glass indices after the manner of Rutherford's thermometer, the instrument might be made to record maxima and minima intensities of light, as well as be more delicate in other respects.

Measured with this instrument, the light necessary to give the mill 240 revolutions per minute does not exceed  $24^{\circ}$ , and is probably less than this, which shows that the theoretical difference of heat necessary to cause the heat-reactions is well within the difference as actually measured, leaving an ample margin for error in the methods of approximation used in the calculation.

In concluding the paper the author claims to have set at rest the only point respecting the explanation of the motion caused by heat which remained doubtful after he had discovered that, according to the kinetic theory, the communication of heat to a gas must cause a force reactionary on the surface, viz. whether this reaction was adequate in amount to cause the results seen to take place.

He adds a suggestion as to a new form of light-mill to have vanes inclined like the sails of a windmill, and not having one side white and the other black, like the light-mills at present constructed, arguing that the forces act perpendicularly to the surface, and in a direction independent of that from which the light comes; so that such a mill would turn like a windmill with the full and not merely the differential effect of the light. Such a mill, he concludes, would furnish another test as to whether or not the force is directly referable to radiation.

### XXXI. *Intelligence and Miscellaneous Articles.*

ON THE ELECTROMAGNETIC ACTION OF ELECTRIC CONVECTION.

BY DR. HELMHOLTZ\*.

I UNDERSTAND by electric convection the conveyance of electricity by the motion of its ponderable bearers. In my last memoir on the theory of electrodynamics†, I proposed some experiments (which were then carried out by Herr N. Schiller) in which the question came into consideration whether electric convection is dynamically equivalent to the flow of electricity in a conductor, as

\* A Report on some experiments carried out by Mr. Henry A. Rowland, of J. Hopkins's University in Baltimore.

† *Monatsbericht* of the Berlin Academy of Sciences, June 17, 1875, p. 405.

W. Weber's theory assumes. Those experiments might possibly have been decisive against the existence of such an action. They were not so; but, on the other hand, through this negative result the existence of the action in question remained unproved. Mr. Rowland has now carried out a series of direct experiments, in the physical laboratory of the University here, which give positive proof that the motion of electrified ponderable substances is also electromagnetically operative. I here remark that he had already conceived and fully considered the plan of his experiments when he came to Berlin, without any previous cooperation on my part.

The moved bearer of the electricity was a disk of ebonite 21.1 centims. in diameter and  $\frac{1}{2}$  centim. thick. It could revolve with great velocity (up to 61 times in a second) about a vertical axis fixed in its centre. The ebonite disk was gilt on both sides; but the gilding was insulated from the axis. Near it, above and below, were placed glass disks of 38.9 centims. diameter, pierced through the middle to admit the axis of the ebonite disk. The glass disks were likewise gilt, in an annular band of 24 centims. external, 8.9 internal diameter; the gilt side was mostly turned toward the ebonite disk. The gilt surfaces of the glass disks were, as a rule, connected to earth; while the ebonite between them, through a point directed toward it at a distance of  $\frac{1}{3}$  millim. from its margin, was in electrical communication with the coatings of a large insulated Leyden battery which served as a reservoir for the electricity. A commutator of a peculiar construction, inserted between them, permitted now the one, now the other coating to be connected either with the ebonite disk or with the earth. In the construction of these parts, iron was avoided.

Close above the upper glass disk an extremely sensitive astatic needle was suspended to an arm fixed in the wall, and completely enclosed in a brass case connected to earth. The two needles were 1.5 centim. long, but at a considerable distance (17.98 centims.) from each other. Their deflections were read off with a mirror and a telescope. The opening in front of the mirror was protected from external electrical influences by a metallic hollow cone. Indeed the electrical charge of the battery and the reversal of the electrification of the ebonite disk gave no perceptible trace of action on the needle so long as the ebonite was stationary.

On the other hand, on swift rotation, even without electrifying, the action of rotation-magnetism was shown, mostly arising from the brass axis of the rotating disk, and considerably diminished by reducing it to 0.9 centim. thickness. The action of the electrification of the disk could be separated from that of the rotation-magnetism by letting-in alternately positive and negative electricity (by means of the commutator above-mentioned) while the velocity of the rotation was maintained unaltered. The displacement of the needle from the position of equilibrium amounted to from 5 to  $7\frac{1}{2}$ , its arc of oscillation on changing the electrification, therefore, to from 10 to 15 scale-divisions. This result ensued in hundreds of observations (which were made with gradually and continually more and more improved apparatus in the course of several weeks), and always in the same direction. The direction of the deflection

of the needle, the length of which was normal to the radius of the disk, was such as would have been produced by a positive electric current flowing with the rotation of the positively charged disk, or against the rotation of the disk charged negatively.

There was no alteration in the action when the gilding of the ebonite plate was removed in a series of radial lines, so that annular electric currents could no longer take place. A thin plate of glass was also inserted instead of the gilt ebonite, and, like the disk of a Holtz machine, could be electrified through points; while close beneath it there was a gilt resting plate connected to earth, in order to fix as much electricity as possible. The direction of the deflections was the same as in the previous experiments; but they were smaller, as the conditions were not so favourable for strong electrification.

In order to compare the electricity carried forward by convection with that which passes in conductors, experiments were instituted in the following manner.

The ebonite disk was gilded afresh, and the gold coating divided, by a series of fine circular lines, into rings insulated from one another. The innermost ring was connected with the axis; the rest could not at any rate become considerably charged without discharging themselves by very short sparks from one to the other. Two electrified plates, each having the form of a sector of a circle, but which did not reach to the axis, were placed, opposite to one another, above and below the rotating plate. Under these circumstances the electricity of the gold rings must have been accumulated by electrostatic induction in the sector covered by the last-mentioned plates, and carried forward convectively. When this electricity was positive, it became free at the fore margin of the induced sector (in the direction of the rotation), while at the hind margin of the same, continually new positive electricity being attached, relatively negative electricity became free.

The positive electricity must, under these conditions, have overflowed from the fore to the hind margin of the sector, for which there were in each ring two paths open, between which it must have divided itself in the inverse ratio of their resistances. If the inducing sector comprises  $\frac{1}{n}$  of the circumference, the resistance of the path in the sector is to that of the path outside of it as  $1:n-1$ ; and therefore  $\frac{n-1}{n}$  of the current returns through the sector, and  $\frac{1}{n}$  outside of it. In the sector a quantity corresponding to the sum of the two currents is carried forward against the current by convection. If, then, a convective motion of electricity acts like a conducted motion, the total motion in the sector is  $1 - \frac{n-1}{n} = \frac{1}{n}$ . But if the action of convective had been greater or less than that of conducted motion, the excess, in one or the other direction, must have been shown on the sector.

The experiments showed that, when the sector was small ( $\frac{1}{8}$  of

the circumference), the small difference between the convection 1 and the conduction  $\frac{1}{3}$  in general could not (or at least not with certainty) be detected, that therefore, with approximate equality of convection and conduction, the electrodynamic effect of the one sensibly neutralized that of the other.

When, however, the sector took in half of the circumference, the here assumed current could be observed even in the free portion of the disk, though the amount was too small for safe measurement.

The observed electrodynamic action being so little in the foregoing experiments, in which the disk was electrified and covered in its whole extent by the induced plates, theoretical calculation of the amount of the action from the known absolute values of the electrodynamic constants promised only approximately accordant values. Nevertheless it was carried out by Mr. Rowland.

The proportion in which the action of the earth's magnetism upon the pair of astatic needles was diminished was ascertained by finding the oscillation-period, first with the needles equally directed, and then arranged astatically.

The value of the function of the electric potential in the Leyden battery, and on the rotating disk, was determined according to the law of spark-length given by Sir William Thomson, which in this case appeared sufficiently accurate. Before and after each experiment, a smaller jar was charged from the battery of nine large ones containing the store of electricity, and on it the length of the spark was determined.

The velocity of the rotation was regulated by the position of the balls of a centrifugal governor, which was applied to one of the more slowly rotating axes. The calculation from the magnitude of the rollers agreed well with the determination by the tone of a siren-disk, which was for some time attached to the most rapidly revolving axis.

In the calculation of the distribution of the electricity on the disk, and its electromagnetic directing-force, the surplus charge present at the margin of the disk was reckoned according to the value which holds for infinitely thin disks, and considered as an infinitely thin thread concentrated at the margin—a proceeding which was in both ways only approximately correct, but, in presence of the minuteness of this portion, was sufficient.

The influence exerted upon the upper needle was about  $\frac{1}{50}$  of that upon the lower. The horizontal force of the earth's magnetism was put equal to 0.182 (using the centimetre, gramme, and second as units); the electrodynamic constant was put by Mr. Rowland, after Maxwell's determinations, equal to 28,800 millions. W. Weber's value would be 31,074 millions. I give below under M. the results calculated with the former value, under W. those calculated with the latter.

The following is the result of the calculation of only three series of experiments conducted under favourable circumstances:—



- (1) Ten Experiments with alternately opposite rotation. In each, three readings, of which the middle one was made with the electrification of the disk opposite to that of the first and third.

Mean difference of the position of equilibrium, in	
scale-divisions .....	6.735
Spark-length .....	0.2845
Electrodynamic force acting on the astatic pair—	
observed ....	0.00000327
calculated, M.	0.00000337
„ W.	0.00000311

- (2) Four experiments, the same.

Difference of the position .....	7.50
Spark-length .....	0.2955
Electrodynamic force—observed .....	0.00000317
calculated, M. ....	0.00000349
„ W. ....	0.00000322

- (3) Five experiments, the same.

Difference of the position .....	7.60
Spark-length .....	0.2926
Electrodynamic force—observed .....	0.00000339
calculated, M. ....	0.00000355
„ W. ....	0.00000328

The accordance may be looked upon as satisfactory in the measurement of a force which amounts to only  $\frac{1}{50000}$  of the force of the earth's magnetism, since in two of the series the observed values fall between those corresponding to the different measured values of Weber's constant.

As regards the signification of these experiments for the theory of electrodynamics, they correspond to the hypotheses of the theory of W. Weber; but they can also be referred to Maxwell's or to the potential-theory which takes account of the dielectric polarization of the insulators. The volume-elements of the stratum of air situated between the resting and the moved plates suffer continual displacements in the direction of a rotation round radially directed rotation-axes. The existing dielectric polarization of these elements will therefore in each material element continually change, while retaining in space the same direction normal to the surface of the electrified disks. The arising and disappearing components of this polarization would constitute the current which is indicated by the astatic pair of needles.—*Monatsbericht der kön. preuss. Akademie der Wissenschaften zu Berlin*, 1876, pp. 211–216.

#### NEW FORM OF COMPENSATING PENDULUM.

BY J. LAWRENCE SMITH, LOUISVILLE, KY.

In the construction of this new form of compensating pendulum, I have taken advantage of the very great expansibility of that combination of sulphur and caoutchouc known as vulcanite or ebonite. Its coefficient of expansion is known to approach that of mercury in the ranges of temperature from 0° to 70° C.

The mechanical arrangement adopted is a very simple one. The rod of the pendulum is of round steel, with an adjusting screw at the lower end: a round rod of vulcanite with a hole in the centre is passed on to the steel rod, fitting it loosely, and being held in place by the adjusting screw. The bob of the pendulum consists of a heavy mass of brass with a hole through the centre large enough to admit the vulcanite, over which it passes, and, by a properly arranged stop, rests on the end of the vulcanite furthest from the lower end of the pendulum; so that any expansion of the vulcanite elevates the brass bob, thus compensating for the downward expansion of the steel rod and brass bob.

There is a simple mechanical arrangement for adjusting the proper difference between the length of the vulcanite and the other parts of the pendulum.

For a second pendulum to an astronomical clock I have used the following dimensions—diameter of the steel rod 6 millims., diameter of vulcanite 25 millims., length of same 165 millims., diameter of brass bob 63 millims., length of the same 156 millims. These dimensions are in no way insisted on as being the best. For a half-second pendulum I have used a steel rod 3 millims. in diameter, vulcanite 11 millims. in diameter and 63 millims. long, brass bob 38 millims. in diameter and 57 millims. long.

I have had one of these pendulums attached to an astronomical clock; and, after adjustment, it has been running four months with very satisfactory results. Should this form of pendulum prove itself constant and correct, it would certainly be a convenient one for transportation, and very much less costly than the ordinary form. And as for the half-second pendulum, in such constant use in mantle-clocks, it will be of the greatest service and not add more than twenty cents cost to the commonest form of pendulum that can be used.

As regards the uniformity of the coefficient of expansion of all vulcanites, of course it is not to be supposed that it can be relied upon; but a very simple method is used to ascertain it for any single specimen, or for a number made of the same lot of material.

I have made experiments on several different specimens; and the results vary little from each other. The range of temperature with which the experiments were made was from zero to 43° C., on a bar 25 millims. in diameter and 304 millims. long, this expanding in length 9–10 millims., making the entire expansion equal to  $\frac{1}{128}$  of the entire length of the rod for a temperature ranging from freezing- to boiling-point, giving as coefficient for linear expansion for one degree Centigrade 0.000079365. This coefficient is seen to be lower than that of mercury; but from the fact that mercury corrects the pendulum by only one half its expansion, and the vulcanite is made to correct it by its entire expansion, the length of vulcanite required is even less than the column of mercury used in the mercurial pendulum. This instrument is one whose use depends on its accuracy of operation after careful trial for some time.—*Silliman's American Journal*, August 1876.

ON THE UNIFORM MOTION OF WATER, BOTH IN SMALL CANALS AND TRENCHES AND IN STREAMS AND RIVERS. BY M. HAGEN.

For the former the observations of Darcy and Bazin on the inlets and outlets of the Bourgogne canal, and for the latter those of Humphreys and Abbott on some of the great rivers of America were taken as a basis. Introducing the simple law

$$c = k \cdot \alpha^x \tau^z,$$

where  $c$  signifies the mean velocity,  $\alpha$  the relative fall, and  $\tau$  the mean radius (that is, the quotient obtained by dividing the area of the cross section by the liquid periphery), and employing the method of least squares, gave, after comparison with various other measurements :—

For trenches,  $c = 4.9 \cdot \tau \alpha^{\frac{1}{3}}$  ;

For rivers,  $c = 6 \cdot \tau^{\frac{1}{2}} \alpha^{\frac{1}{3}}$ .

The numerical coefficient holds in the first case for any measure whatever ; but in the second it holds for English and Rhenish measurement in feet, while for metres it changes into 3.34.

As soon as  $\tau = 1.5$  foot or  $= 0.47$  metre, both expressions give the same result ; at this point, therefore, there is a transition from the one law to the other.—*Monatsbericht der k. preussischen Akademie der Wissenschaften zu Berlin*, 1876, p. 243.

#### A GLASS CIRCLE FOR THE MEASUREMENT OF ANGLES.

BY LEWIS M. RUTHERFURD.

At the summer meeting of the National Academy of Sciences in the year 1866, I described the micrometer which I had constructed for the measurement of astronomical photographs. It was capable of measuring angles of position, and also distances in directions at right angles to each other. These last measurements were made by aid of screws arranged after the manner of those of an ordinary slide rest : these screws were constructed with great care ; and I had good reason to be satisfied with the smallness of their errors.

At the spring meeting of the Academy for the year 1870, I explained that I had been obliged to give up the idea of using screws on account of the rapid changes in their errors caused by friction and consequent wear ; and I then stated that I intended to discard the screw and the compressed slide, and substitute for them a divided glass scale, to be read by a micrometer-microscope, and a gravity-slide with one V and one flat slide. This intention I carried out during the year, the new form being first used about the month of March 1871. It has been constantly used since that time, and continues to give great satisfaction. The success of this divided glass scale confirmed me in a determination of long standing, to try the experiment of substituting a glass circle for one of metal in some instrument for the measure of angles of precision.

Two years' absence in Europe and other occupations conspired to postpone the execution of this plan until the past winter, during which it has been realized with what seems to me the most pro-

missing success. I had in my possession a spectrometer by Bruner of Paris—his small model, similar to the one used by Mascart, and figured in his paper on the measures of wave-lengths. This instrument has a good steel centre, and was furnished with a circle divided on silver, reading by means of the verniers to 10". The diameter of the circle is small, not quite 7 inches; and the inability to read smaller angles has always been its weak point. I have substituted for this metallic circle one of glass about 10 inches in diameter, divided by Mr. Stackpole to ten minutes of arc, and read by two micrometer-microscopes magnifying seventy-five times; each revolution of the screws being equivalent to one minute, the drums, being divided into sixty parts, read to seconds with easy estimate of fractions; each degree-line is numbered so as to be visible in the field of the microscope. I was able to furnish to Mr. Stackpole a well-tried diamond which has made lines of the greatest delicacy, being much finer, as seen in the microscopes, than the spider-lines, by means of which the bisections are made. The advantages of this system are obvious, viz.:—perfection of surface, permitting a line of any desired fineness; facility of illumination, permitting the extension of the power of the reading-microscopes to several hundred times; smallness of dimensions, and consequent cheapness and avoidance of almost all the questions of flexure and local effects of temperature.

I am convinced from the ease with which one second is read on my instrument, with microscopes only  $4\frac{3}{4}$  inches long, including objectives and eyepieces, that, upon a circle of 15 inches provided with powerful microscopes, greater precision could be attained in the reading of angles than with the largest metallic circles now in use.

For the purpose of showing the degree of precision attainable, I add two series of bisections of lines on the circle made by myself, and two made by a lady, marked respectively R. and M.:—

R.	R.	M.	M.
7.5	1.8	23.6	11
7.4	1.3	23.6	11.2
7.7	1.9	23.3	11.8
7.4	1.8	23.8	11
7.3	1.8	24.5	10.9
7.6	1.8	23.9	11.5
7.7	1.9	23.7	11.5
7.4	2	24	11.5
7.6	2.3	23.9	11
7.8	2.3	24.3	11.4
Mean ..	7.54	23.86	11.28

It will be readily seen that the probable error of any single reading in any one of these series is considerably less than half a second, while the probable error of the mean of any series is a much smaller fraction.—Silliman's *American Journal*, August 1876.

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[FIFTH SERIES.]

OCTOBER 1876.

XXXII. *On the Transformation of Gravity.*  
By JAMES CROLL, LL.D., F.R.S.\*

**G**RAVITY in relation to Correlation.—Is gravity convertible into other forms of energy? Can gravity be converted into heat, electricity, magnetism, &c.? or can those forms of energy be converted into gravity? One, I presume, may read all that has been written on correlation and conservation of energy without finding a clear, distinct, and satisfactory answer to the question. Few will admit that gravity forms an exception to the great principle of correlation, standing isolated from all other forms of energy. It may be true that gravity cannot be directly transformed into heat, electricity, magnetism, chemical affinity, &c., nor those forms directly transformed into gravity; but nevertheless the thing may be done indirectly. It is well known that if energy (no matter under what form) can be converted into motion or mechanical work, it can then be transformed into other forms of energy. For example, if heat is made to perform mechanical work, say to drive a machine, then that machine may be made to generate electricity, and the electricity may in turn be converted into other forms of energy. Or electricity may drive the machine, then the machine may generate heat. The heat is here not directly converted into electricity, nor the electricity directly into heat; but it is done indirectly. The heat is first converted into mechanical energy and then into electricity. The electricity is first converted into mechanical energy, then into heat. Now gravitation will drive a machine

\* Communicated by the Author, having been read before the British Association, September 1876.

as effectually as either heat or electricity; and the energy thus employed is as really and truly converted into mechanical energy as was the heat or the electricity in the cases to which we have referred. An electrical machine driven by water-power will as effectually produce electricity as though it were driven by steam-power or by horse-power; and the electricity produced will as truly be derived from gravity in the one case as it would be from heat or from animal power in the other cases.

If then it be admitted, as it must be, that in the case of the electricity produced by the machine driven by steam-power the electricity previously existed under the form of heat, for the same reason it ought to be admitted that the electricity produced by the machine when driven by water-power previously existed under the form of gravity.

We may now suppose the process reversed and electricity to be the motive power. Conceive a machine of some kind or other driven by electricity, and this machine to be employed in the production of heat, say by friction, and to be employed also in raising water. It will be universally admitted that we have in the one case electricity converted into heat; but it is equally evident that we have in the other case electricity converted into gravity. If the electricity produced by the descent of the water be gravity transformed into electricity, then the ascent of the water produced by electricity must be electricity transformed into gravity; for it is the same process merely reversed.

*Gravity in relation to Conservation.*—If gravity be correlated to other forms of energy, it must, like them, come under the great principle of conservation. But here we enter upon debatable ground. It is admitted that gravity can perform mechanical work, and the mechanical work can be converted into other forms of energy. Here we have correlations; but it is generally denied that there is a decrease or loss of gravity resulting from such transformations. But this appears to me to be a virtual denial of the principle of conservation. To suppose a steam-engine to perform the work of raising the loaded piston, without the steam sustaining any loss of heat, would be universally admitted to be a violation of the principle of conservation. Every one would maintain such a thing to be impossible, and that the steam must not simply lose heat but lose an amount equivalent to the work performed. Apply the same mode of reasoning to gravity. If gravity perform mechanical work in pulling down the piston, then there must be a loss of that form of energy equivalent to the work performed. But it is not admitted that gravity is

diminished or is weakened by the work which it performs. If a stone one pound in weight fall from an elevation of 1000 feet, one thousand foot-pounds of work will have been performed by gravity on the stone. The stone on reaching the ground is possessed of that amount of energy in the form of motion, all of which has been derived from gravity. But gravity, it is nevertheless maintained, has sustained no loss in communicating this amount of energy to the stone.

The reasons which appear to have led to this opinion are, I think, mainly the two following:—1. It has been assumed that the weight of a body is not affected by the work which it performs. 2. The force by which bodies are drawn towards each other does not diminish as they approach, but on the contrary increases. The mutual attraction of the stone and the earth (gravity) does not diminish as the stone descends, but the reverse; for the stone on reaching the earth's surface is attracted with greater force than it was before it began to descend. From this it is naturally inferred that, whatever may have been lost in the imparting of motion to the stone, it cannot have been gravity.

In reference to the first of these reasons, it may be remarked that although what we call the permanent weight of a body is not affected by the work performed, still it does not follow that the weight may not be affected at the time the work is being performed, and still less that there may not be a loss of gravity resulting from the performance of the work. And in regard to the second reason, it does not necessarily follow that, because the force of gravity acting on bodies increases as they approach, there is not a loss of force resulting from their approach. A person, for example, may be growing richer every day, and yet at the same time his daily expenditure may be on the increase. Two electric currents parallel and flowing in the same direction mutually attract each other; and this attractive force increases as they are drawn together; but we know that there is at the same time a loss of attractive force resulting from their approach. When the two currents are in the act of approaching towards each other, an induced current takes place in each wire in the opposite direction to the primary current, the effect of which is to weaken the primary currents, and consequently to diminish the force of their attraction; so that when the currents are approaching each other the force of their attraction at any given moment is somewhat less than it would otherwise be were the currents stationary. The same is true of magnets. When an electromagnet is drawing towards it a bar of soft iron the attractive force of the magnet for the iron increases

as the bar approaches ; but there is, notwithstanding, a slight loss of attractive force resulting from the approach of the bar. The attractive force of the magnet for the bar when it is in the act of approaching is less at any given distance than it would otherwise be were the bars stationary at that distance ; for the approach of the magnetized iron weakens the magnet by weakening the current surrounding it. It is also probably true, though it cannot be so easily proved, that a permanent magnet is weakened by the approach of the magnetized iron. May not the same be true of bodies approaching each other under the mutual influence of the attractive force of gravity ? May not a stone when in the act of falling be acted upon by gravity with less force at any given moment than it would be were the stone at rest at that instant ? The point has never yet been determined either by experiment or by observation.

The fact that the attractive force of bodies increases as they approach towards each other cannot, therefore, be regarded as evidence that no loss of force results from their approach. It may be here also remarked that neither can the fact of this increase of attractive force as bodies approach be regarded as in any way opposed to the law of conservation ; for, as was pointed out by Professor Rankine in his reply to Faraday's objection \*, no law of conservation is applicable to the simple force or tendency of two bodies to approach each other,—the physical reason being that the things conserved cannot be two forces ; for the one is never directly convertible into the other. What must be conserved is the force, and the work which it performs. But if the force of gravity does not sustain any loss as work is performed by it, what, then, is it that is supposed to sustain the loss ? Some form of energy must diminish as work is performed ; and if it be not gravity, it must be something else.

The generally received explanation is this : when a body is projected upwards, the potential form of energy into which the upward motion of the body is transformed does not consist in the simple force of gravity or tendency of the body to descend, but consists in this force or tendency multiplied by the distance through which it is capable of descending. If a stone one pound in weight is at an elevation of 100 feet, then one pound multiplied by 100 feet gives 100 foot-pounds of potential energy possessed by the stone at that elevation ; and this is usually termed the *energy of position*. Again, when the body has descended to within, say, 20 feet of the ground, it possesses only 20 foot-pounds of potential energy in virtue of position ; for one pound multiplied by 20 feet

\* Phil. Mag. [IV.] vol. xvii. p. 250 (1859).



gives 20 foot-pounds ; and when the ground is reached the potential energy is gone ; for although the force of gravity is not diminished, yet the potential energy is consumed, there being now no space through which the force can act. Kinetic energy thus generated being measured by the mass of the stone multiplied by the half-square of its velocity, the sum of the potential and the kinetic energies must therefore always remain constant. When the stone reaches the ground it is in possession of the 100 foot-pounds of energy in the form of motion. When it was halfway down it possessed 50 foot-pounds in the kinetic form of motion, and another 50 foot-pounds in the potential form ; for, having another 50 feet to fall, its potential energy was  $1 \times 50 = 50$  foot-pounds.

This mode of viewing the matter, it is perfectly true, completely meets the mathematical and mechanical conditions of the problem ; but for this very reason it seems to me to hide somewhat the real physical nature of the process.

On a former occasion \* I briefly endeavoured to show that the fundamental objection to this mode of explanation is that it makes *space* a form of energy, whereas, when we analyze the process properly, we find that time and space can, in the very nature of things, be nothing more than conditions. The object of the present paper is to discuss more fully this objection.

*Time and space are but conditions in the process of transformation.*—Space and time are conditions, but conditions absolutely necessary, to the transformation of potential energy into kinetic and of kinetic energy into potential ; but they themselves cannot be forms of energy. But if it be true that the mere force of gravity or tendency of the stone to fall to the ground is not the potential energy, but that this potential energy is the force  $\times$  the space through which it can act, then space must become a form of potential energy. This is evident ; for the potential energy in this case consists of two factors, one of which is the *space* through which the force acts. It thus becomes just as much a form of energy as the other factor, viz. the force.

The reason why this mode of explanation fulfils the mathematical conditions of conservation is obvious. Space, like time, being a condition absolutely essential to the transformation of potential energy into kinetic, it follows that, no matter how much potential energy may be in the raised stone, no transformation can possibly take place if the space condition is not supplied, and the amount of transformation will just be in proportion to the space supplied. The stone may be raised to

\* Phil. Mag. December 1867.

any height ; but if it can only descend through one foot and no more, one foot-pound of the potential energy stored up in the form of gravity can be transformed into the kinetic form ; but if two feet, three feet, ten feet, fifty feet, and so on be supplied, two foot-pounds, three foot-pounds, ten foot-pounds, and so on of potential energy will be converted into kinetic. The amount of transformation will be in proportion to the amount of the space condition supplied. When the stone reaches the ground no further transformation can take place, not because the potential energy is exhausted, but because no further space condition is available. But were a hole dug through the earth the stone would continue its descent, and transformation would go on till the earth's centre was reached, when the entire amount of potential energy of gravity acting on the stone would have disappeared and no further transformation of potential energy into kinetic would be possible—not, however, as before, because there was no more space condition, but because there would be no more gravitating force acting on the stone.

It is gravity, and gravity alone, which imparts motion to the stone. No work is performed on the stone by space ; space and time merely supply the conditions for the work being done. That which, in the potential state, becomes transformed into kinetic energy must be gravity, not space. The kinetic energy which appears as the stone descends must have previously existed in the form of gravity, not as space. This truth is so self-evident that it can hardly be denied by any one who will reflect on the subject. But if the kinetic energy be derived from gravity, then there must be a decrease of gravity proportionate to the increase of kinetic energy, or else the principle of conservation is violated. If a force be transformed into something else, say into kinetic energy, then it cannot be what it was before transformation, but must be what it is transformed into, viz. kinetic energy\*.

\* Mr. Lewes, in his 'Problems of Life and Mind' (vol. ii. p. 353), lays it down as a fundamental principle that force is invariant and cannot be expended or even transformed. Every unit of force, according to him, remains unalterably the same in amount. His views seem to be :—that when the forces balance one another there is rest, static equilibrium ; but when there is an excess of pressure in one direction, motion or *vis viva* results ; but the forces themselves are invariant, and never increase or diminish. This is just the fundamental error which I have been combating. Force, I consider, is energy in the potential form ; motion is energy in the kinetic form. Neither the one nor the other remains constant, but both are in a state of change. That which remains constant is the sum total of both. And as the one is convertible into the other, it necessarily follows that the one must decrease as the other increases, and *vice versa*.

But it is not in reference to gravity alone that this space condition is essential to the transformation of potential into kinetic energy. It is, as we shall shortly see, a condition absolutely necessary to the transformation of energy under every possible form. In the unbending of a spring the amount of work which can be performed is proportionate to the pressure of the spring multiplied by the space through which the pressure can act. The potential element, which in this case decreases as work is performed, is the tension or elastic force of the spring. Space is necessary simply to allow this tension to expend itself in work. It is this tension, not space, which reappears as work or kinetic energy. Take also the case of the steam-engine. The amount of work which can be performed is proportionate to the pressure of the steam in the piston multiplied by the space through which the piston can act. When the piston reaches the end of the cylinder, no more work or transformation of potential energy into kinetic is possible, not because there is no more pressure or potential energy to be transformed, but because there is no more space to allow of transformation. But in order to secure an inexhaustible supply of the space condition the ingenious device of "reversing the stroke" has been adopted—that is, of transferring the pressure to the opposite side of the piston when it reaches the end of the cylinder. By this device as much space becomes available as would be were the cylinder made a thousand miles in length. In fact the space condition becomes thus illimitable. But, be it observed, although the work performed is proportionate to the pressure of the steam multiplied by the space through which the pressure acts, still it is the pressure, and the pressure alone, which performs the work. True the pressure in the cylinder may be kept constant; but this can only be done by admitting more steam from the boiler. The energy which performs the work, or, in other words, which becomes transformed into work, is altogether derived from the boiler; the reversal of the stroke simply supplies the space condition for transformation. The energy which disappears as work is performed is *heat*, not *space*. The work performed represents so much heat lost. The work is the equivalent of the lost heat, not of the space passed over. Whatever we have gained in work we have lost in heat.

Every physicist will readily admit this conclusion in reference to an engine propelled by heat, because to suppose that that could perform mechanical work without sustaining loss would in fact be a violation of the principle of conservation. But gravitation turns our water-wheels and grinds our

corn, performing mechanical work as truly as it is done by heat ; and all this is supposed to take place without gravitation sustaining any loss. The only thing supposed to be lost is position or space to pass over. Gravity is the real agent that performs the work ; and yet it is supposed to sustain no loss thereby. Heat in the steam-engine is the agent that performs the work ; but it would be considered absurd to say that no heat was lost, and that what was lost was simply space in the cylinder, along which the piston could move. The absurdity seems, however, to be quite as great in the one case as in the other ; and it appears as much a violation of the principle of conservation to suppose that gravitation could perform work without loss as that heat could do so. That form of energy called heat is *weakened* by the act of raising the loaded piston against gravity. May not that form of energy called gravity be also *weakened* by the act of pulling the piston down ?

It is true that the pressure of the steam has a dynamical origin, and that in the performance of work there is a loss of pressure resulting from a loss of molecular motion. May not the same be equally true of gravity ? In fact, if gravity has a dynamical origin, it must hold equally true of gravity and of heat. If the force of gravity, for example, results (as it, however, probably does not) from the impact of ultramundane corpuscles, as advanced by M. Le Sage, then there must be a loss of force resulting from a loss of corpuscular motion when work is performed, as truly as in the case of the steam-engine ; in fact the two cases are exactly parallel.

That space is not a *form* of energy but simply a *condition*, becomes further evident from the nature of kinetic energy in the form of motion. The kinetic energy of a moving body is equal to its mass multiplied by the half-square of its velocity. Now it must be observed that the energy possessed by the body depends upon its velocity, and is entirely independent of the amount of space passed over. A body moving with a certain velocity possesses the same amount of energy whatever may be the space passed over. A one-pound weight moving at the rate of 1000 feet per second possesses 15,625 foot-pounds of energy, whether it moves over one thousandth of a hairbreadth or over one thousand miles. It would not possess any more energy without a change of velocity though it were to continue its course onward in space through all eternity. The energy possessed is the same whether the space passed over be infinitely great or infinitely small, which would not be the case if space were itself a form of energy.

In order to perceive more clearly the true relationship of

time and space to the transformation of energy, it will be necessary to treat the subject in a more general and abstract form, so as to subject the process to a closer analysis.

*Time and Space in relation to EXISTENCE.*—If we assume the objective reality of time and space, we must affirm that every thing which *exists* must exist in time and space. But although existence necessarily implies time and space, it does not necessarily imply any measurable quantity or *amount of time and space*. A thing which exists, no matter what it may be, exists in the absolute and indivisible moment. Continued existence, of course, necessarily implies an amount or measurable quantity of time, but not more existence abstractly considered.

The same does not, however, hold universally true in regard to space ; for whether the existence of a thing necessarily implies an extension or measurable quantity of space depends upon whether the thing possesses extension or not. For example, thoughts, feelings, emotions, &c., and other mental phenomena must exist in space, but do not require extension in space. Length, breadth, and thickness have no application to thought or to feelings. A mathematical point, for example, has position in space, but it does not occupy space.

*Time and Space in relation to CHANGE.*—Lapse of time is necessarily implied in the very conception of change. Change is the passing from one state to another. The thing changed is *first* in the one state, and *afterwards* in the other. It cannot be in the two different states at the same moment.

Change does not, however, necessarily imply a quantitative amount of space as it does of time. Change implies succession of moments of time, the one following the other ; but it does not necessarily imply a succession of positions in space. There can be no change without change of position in time ; but there may be change without change of position in space. Change of thought, feelings, &c. do not imply change of position in space. It may be true, or it may not, that mental change cannot take place without some change in our material organism (say, of the brain) ; but nevertheless the most rigid materialist will admit that change of *thought itself* does not imply change of position in space. Even in material objects change does not always imply change of position. A body may also be conceived to increase or decrease in density without any change in the position of its particles. Change of *intensive* quantity does not imply change of position in space.

*Time and Space in relation to TRANSFORMATION OF ENERGY.*—It will be perceived at once that lapse of time is necessarily implied in the very conception of transformation. Transfor-

mation is a change from one form to another. There is *first* the one form, and then the other ; or, in other words, there is first the one form, and *afterwards* the other. The two are not simultaneous. It becomes the one by ceasing to be the other. Transformation without lapse of time is both inconceivable and a contradiction in terms.

If we leave out of consideration mental phenomena, we may affirm that transformation of energy without transposition in space is just as impossible as transformation of energy without transposition in time ; only it is not so directly obvious in the one case as in the other. That transformation of energy necessarily implies change of position in space may be proved from general considerations alone, without reference to special cases.

Transformation of energy must consist either (1) of change of potential energy into kinetic, or (2) of kinetic energy into potential, or (3) of one form of kinetic energy into another form of kinetic energy, or (4) of one form of potential energy into another form of potential energy. There can be no other form of transformation beyond these ; for they comprehend the whole field of the possible. Now it is obvious that the first three forms of transformation imply change of position in space ; for kinetic energy implies motion in some form or other, and motion implies change of position in space. The fourth and last form of transformation, viz. of one form of potential energy into another, were it possible, might, it is true, be conceived to take place without change of position ; but there is evidently no such thing in nature, so far as is yet known, as one form of potential passing directly into another form. It is true that the potential energy of one form may be transformed into potential energy of another form ; but it must, in the first instance, become kinetic before it assumes the second form. Static electricity, for example, may be transformed into magnetism, but not so directly. It must first assume the dynamic form before it can become magnetism. A magnet will magnetize a piece of soft iron, but before the iron assumes the magnetic or potential state a molecular change takes place. The first effect produced on the molecules of the iron is dynamic before they assume the static or magnetic state.

The conclusions to which we are led may be stated as follows :—

1st. Under every possible circumstance time and space are absolutely necessary as *conditions* to the transformation of energy from one form to another—the physical reason being that there can be no transformation without motion, and there can be no motion without time and space as conditions.

2nd. In every possible case the amount of transformation is limited by the extent of the space condition available for the forces to act. We have seen that in the steam-engine, for example, the amount of energy in the form of heat that can be converted into mechanical work would be limited by the length of the cylinder, were it not that the stroke of the piston is reversed; for when the piston reached the end of the cylinder no further transformation of energy could take place. But the reversal of the motion of the piston affords an unlimited amount of space condition; consequently, so far as space is concerned, there is no limit to the amount of heat which may be converted into work in a steam-engine. The same thing is also true of the molecules of the steam itself. They could perform no work on the piston unless they had space to allow of motion. But as their motion, like that of the piston, is to and fro, there is no limit, so far as space is concerned, to the amount of energy which they may lose in work.

But gravity is a force which cannot act to and fro. The amount of energy which gravity can lose is limited by the distance between the gravitating bodies. When the space between them is traversed and the bodies are in contact, there is then no space condition, and further transformation of energy into work is impossible.

*Energy of Position.*—What is meant by energy of position must simply be that a body is in such a position that *force* may be converted into kinetic energy or energy of motion. It means that the force is in a position, so far as the space condition is concerned, of being transformed into kinetic energy. We do not speak of the piston of the steam-engine being possessed of energy of position. We have no occasion to do so, simply because it is always in possession of energy of position. But we speak of the weight when raised and the bow when bent as possessing energy of position; for it is only when they are in this condition that force can be converted into motion. The generally received meaning, however, of these terms is not only erroneous and misleading, but hides a most important physical truth. For when it is said that a body possesses energy of position, it is not meant simply that the body, in regard to space, is in a position to allow of force being transformed into energy of motion; for it is not supposed that force is transformed at all, the only thing changing being simply the position of the body. When a stone, dropped from a height, is being pulled to the ground by the force of gravity, it is supposed that there is no loss or consumption of gravity; the only thing lost which can possibly be regarded

as the equivalent of the motion gained, is simply the distance between the stone and the ground. Consequently space is not only made a part of the potential energy, but the only part out of which the kinetic energy has been generated—space or distance from the ground being the form in which the energy of the falling stone previously existed.

*Gravity a Space-pervading Force.*—The fact that gravity increases inversely as the square of the distance may be regarded as evidence of the truth of the views advocated by Faraday\*, Waterston†, and others, that it is a force pervading space external to bodies, and that on the mutual approach of the bodies this force is not increased as is generally supposed, but the bodies merely pass into a place where the force exists with greater intensity; for in such a case the intensity of the force, in the space external to any body, is inversely as the square of the distance from the centre of convergence of these lines of force. When a stone, projected upwards, recedes from the earth, its *vis viva* is transferred to space, and exists there as gravity. When the stone approaches the earth, the force existing in space is transferred back to the body and reappears as *vis viva*.

“The integral of gravitation,” says Mr. Waterston, “is a function of space. . . . Each element of radial distance has associated with it a fixed element of mechanical force, to be given or taken from all bodies traversing it.”

*Gravity supposed to be an Impact or a Pressure.*—It is now generally admitted that the conception of *attraction* does not represent the *modus operandi* of gravitation, because attraction implies action at a distance, or, in other words, that a thing acts *where* it is not, which is just as impossible as that a thing can act *when* it is not. Gravity, in all probability, is of the nature of an impact or a pressure. Some of our most eminent physicists state that the force of gravity must either result from impact of ultramundane corpuscles, in some respects analogous to that of the particles of a gas (which has been found to be capable of accounting for gaseous pressure), or it must result from difference of pressure in a substance continuously filling space, except where matter displaces it.

That gravity is a force of the nature of pressure is, I think, beyond all doubt; but that this pressure results from the impact of corpuscles or from difference of pressure in a substance filling space is purely hypothetical. Why not assume it to

\* Phil. Mag. April 1857; Proceedings of the Royal Institution for 1855.

† Phil. Mag. [IV.] vol. xv.



be a force without calling in the aid of corpuscles or a medium filling space?

On a former occasion \* I endeavoured to show at considerable length that our inability to conceive how force can exist without a material medium has its foundation in a metaphysical misconception.

Force cannot be conceived to exist of itself; for it is not a substance, but must be either the property of a substance or the effect of a cause. But there is no *à priori* necessity for forming any conception of the nature of the substance of which it is the property, or of the cause of which it is the effect. This is not all. To suppose that the substance of which the force is the property should be some material tangible thing such as a corpuscle, is to suppose that we must not only know the nature of the substance, but should also be able to form a *sensuous representation* of it. Here is a double error; for it is only the properties that are cognizable through the senses; or, in other words, we know substance only through its properties, or in the way that it manifests itself. Every thing in the corpuscle, for example, which is manifested to the eye, to the ear, or to the touch, is just as much a property or an effect as is force itself. All that the intellect demands is that the force of gravity be the force of *something*; but what that something is other than a something manifesting itself as force we have no necessity for knowing. It does not follow that this something should have the additional properties of hardness, length, breadth, and thickness, &c., so that it may be designated a corpuscle, a material particle, or an elastic medium &c.

*Gravity as a Retarding Cause.*—I shall now briefly refer to a curious consequence which appears to follow from the impact theory, independently of any considerations which have been here advanced regarding the transformation of gravity. If gravity results from corpuscular impact, it follows, although the contrary seems to have been generally assumed, that the force of impact will be greater when a body is moving in opposition to the corpuscles than when moving in the same direction, unless we assume, what would be absurd, that they move with infinite velocity. The collision in the case of two trains meeting each other is more severe than in the case of the one overtaking the other.

It therefore follows that, even though the atmosphere offered no resistance, a body projected vertically upwards would not return to the earth with absolutely the same velocity as it left. A stone, for example, projected upwards against an excessively

\* Philosophy of Theism. Ward and Co.: 1857.

heavy shower of hailstones, will, irrespective of resistance from the air, return to the ground with a certain loss of motion, because the force of impact of the hailstones will be greater on the stone when ascending than when descending. Now if we substitute gravific corpuscles for hailstones we shall have the same result, though, of course, to a far less extent, owing to the enormous velocity of the gravific corpuscles compared with that of the hailstones. But unless these corpuscles move with infinite velocity, the force of impact cannot be absolutely as great on the descending as on the ascending stone; and if so it cannot return to the earth with absolutely the same velocity as it left. There must be a loss of motion, however small that loss may be.

In all probability this is a point too nice ever to be determined by experiment. This loss of motion might, however, if real, be detected in the case of comets or planets with very eccentric orbits. The tendency of this cause would be to produce a shortening of the major axis of the orbit of the planet or comet, and of course a corresponding decrease in the period of revolution.

If gravitation were an impact, no planet nor comet could move everlastingly in an elliptic orbit. But the mutual disturbing forces of the planets will always maintain them in elliptic orbits; and it would therefore seem to follow that gravity alone, without any resisting medium, would ultimately bring the planets to the sun.

The same results will follow on the supposition that gravity is caused by unequal pressure in a substance continuously filling space.

XXXIII. *On the Dynamical Signification of the Quantities occurring in the Mechanical Theory of Heat.* By C. SZILY, of Buda-Pesth\*.

ONE of the most important physical fundamental notions is that of temperature; all the quantities of the theory of heat, nay, every physical quantity stands in relation to this; and yet there is perhaps not a single notion in the entire range of physics which has been so indefinite and obscure. Both the rational definition and the measurement of temperature have for their sole basis the frequently discussed but not

\* Translated from a German version, communicated by the Author, of a memoir which appeared in the Hungarian *Műegyetemi Lapok*, vol. i. p. 165. On perusing the proof sheet of the English translation, the author has made a few modifications, and inserted the results of some of his new investigations on the subject.

yet duly elucidated second proposition of the mechanical theory of heat. Accordingly a rigorous, indisputable proof, a purely dynamical demonstration of the Second Proposition is of pre-eminent interest, not merely for the theory of heat, but also for every department of physics.

Of late years it has already been repeatedly attempted to deduce the Second Proposition without the aid of thermic axioms or hypotheses, merely resting it upon mechanical principles or at least purely mechanical assumptions. In the year 1866, Boltzmann showed that, for a system of material points, one can, on certain special hypotheses, construct a dynamic proposition which in form and meaning appears to be identical with the second proposition of the mechanical theory of heat. In 1871, Clausius somewhat extended and generalized the proposition. In the same year the author of this submitted to the Hungarian Academy a memoir in which he called attention to the fact that the dynamic proposition of Boltzmann and Clausius, at least in form, agrees with Hamilton's principle, whence he inferred that the Second Proposition plays the same part in thermodynamics that Hamilton's principle does in dynamics.

All these dynamic deductions, however, although free from thermic hypotheses, were not free from mechanical assumptions; and on this account they were inadequate to excite absolute confidence in the universal validity of the proposition in question. This circumstance determined me to investigate the question afresh (in a memoir submitted to the Hungarian Academy of Sciences, May 10, 1875\*), and, resting on an almost prophetic utterance of Rankine's, endeavour to deduce the Second Proposition directly from the principle of the conservation of energy without making any further mechanical assumption.

I must confess that in this attempt I at that time encountered very great difficulties: the thoughts move rather heavily; and the deduction rests on perhaps a not quite indisputable basis†. Since then I have been continually pursued by the thought whether the deduction could not be effected more naturally, simply, and secure against every objection. I finally came to the conviction that the chief difficulty consists in this—that *the mechanical conceptions which have been formed*

\* Phil. Mag. [V.] vol. i. p. 22; Pogg. Ann. Ergänzungsband vii. p. 154.

† Thus, for example, in the May Number of the Philosophical Magazine the remark of Mr. Nichols, that in the memoir in question no solid reason was given why the differential of the quantity of heat communicated is to be regarded as a mean value of energy-differentials, is perfectly just.

*of thermic quantities are inadequate, and, taken strictly, are not even correct.* It became evident that, before all, these conceptions must be sifted, and, indeed, in part generalized, in part rendered more precise. I now resolved to give up many of the usual ideas, and to substitute for them more general and, in my opinion, more precise notions. Arrived at this point, the deduction was immediately found to be more simple, more natural.

I now make public the results which I have hitherto attained, viz. (1) the necessary modification of the notions in the theory of heat, (2) the dynamic deduction of the Second Proposition carried out on the basis of the altered notions, (3) conclusions resulting from the new conception.

### 1.

One of the first things assumed in the mathematical treatment of the theory of heat is that the thermal state of a body is constant so long as its volume and temperature remain unchanged. Since in this is given merely a one-sided, and moreover a rather loose definition of that unknown something which is usually named "constant state," taken as it stands we can offer no objection to it. It has, however, been customary to add that it is *only* when the volume and temperature of a body remain continuously constant that its thermal state can and ought to be regarded as unchanged, and that, with any (even an infinitesimal) change of the volume or the temperature, a corresponding alteration of the thermal state ensues. But is this correct? If we adhere to this definition of constant state absolutely, can we presume that there exist bodies the state of which can in this sense be regarded as invariable? For my part, I believe that there does not exist in nature a body of which it could be assumed that its state, thus defined, remains constant, even during only an infinitesimal space of time.

For every body can be considered as an aggregate of numberless, but not infinitely many, material points in continual motion, under the influence of external and internal forces, according to certain unknown laws. Let us picture to ourselves a body with its numberless molecules and the proportionally large interspaces between them. Let us contemplate this little world at a certain instant,  $t=0$ . Each molecule has at this instant a certain position, a definite velocity and acceleration; and it may happen also that the velocity of many a one is just zero. And now let an indefinitely short time  $dt$  pass, after the lapse of which we will contemplate the body again. We perceive an entirely changed constella-

tion? Nearly every molecule has an altered position, velocity, and acceleration; those only have meanwhile continued in the same place and situation whose velocity happened previously to be 0: here the molecules have become scattered; there they have moved closer to one another; here is rarefaction, there condensation perceptible. This is apparent not merely in the interior, but also at the surface of the body; there also some of the molecules have moved further apart, while others have come closer together. At the next moment again another grouping shows itself; and so it goes on in constant change.

Now what is the consequence of this continual variation? As the number of the molecules, though very great, is not infinite, and we will provisionally suppose that no unparalyzed external forces cooperate which could disturb the spontaneous variation, there is just as much *probability* that, at any moment previously selected, the partial rarefactions will predominate, as that the reverse will be the case; and therefore that in an indefinitely short period chosen the sum of the expansions or that of the contractions will predominate is equally probable—supposing, I repeat, that no unparalyzed external forces act on the body. Let us assume that at the moment chosen expansion predominates, so that in that element of time the volume becomes greater. Let us then consider the body again after a while; it may just as well happen that now the contractions predominate and therefore the space occupied is diminishing. As, however, the successive changes can never take place abruptly, all the gradations of both expansion and contraction must occur, and in a continuous series, between the limits determined by the given ratios. Accordingly the volume will during a certain time continually increase until it reaches an extreme limit of expansion corresponding to the circumstances; it then remains an instant unaltered, and thenceforward diminishes uninterruptedly till it reaches the limit of condensation, when it again remains a moment unchanged, to expand again and repeat the performance we have described. It would certainly be a very remarkable and extremely rare chance, if the partial expansions and contractions were *at every moment* to exactly compensate one another. It is far more likely that *the volume is subject to a continual variation, a constant pulsation*. These oscillations are as little perceptible to the senses as the motions of the molecules by which they are brought about. After each complete oscillation the body returns again to its previous phase. If we denote by  $i$  the period of a complete oscillation, and by  $dv$  the actual variation of volume in the time  $dt$ , then the total change of

volume during the period of a complete oscillation is

$$\int_0^i \frac{dv}{i} \cdot dt = 0$$

or

$$\frac{1}{i} \int_0^i dv \cdot dt = 0.$$

When, therefore, it is said that the space-content of a body is constant, this properly signifies only that the sum of the variations of volume during a complete oscillation is nil.

If it be admitted that the body, in relation to these changes of volume, can be regarded as perfectly elastic, and we assume that no unparalyzed external forces alter the amplitude or the period of the oscillations, then on the one hand the duration of an oscillation, and on the other the mean volume for this period, remain constant; or

$$i = \text{const.},$$

and

$$\bar{v} = \frac{1}{i} \int_0^i v \cdot dt = \text{const.}$$

Therefore, if it is said the volume is constant, this by no means signifies that the volume undergoes no variation, but merely that the mean value of the volume remains unchanged.

I think the justification of this conception can hardly be questioned.

But as soon as it is allowed that the space-content is subject to continual alterations, it must also be admitted that the body is continually performing work against the forces acting upon it, now positive, now negative, although its volume is, in the ordinary sense, called constant. It hence follows, not that the work done,  $dW$ , in the time-element  $dt$  is nil, but merely that the mean value of  $dW$  during a complete oscillation is equal to nil, or

$$\overline{dW} = \frac{1}{i} \int_0^i dW \cdot dt = 0.$$

But whence does the body take, and to what does it apply, the energy which comes into play in this alternate performance and consumption of work? On the one hand the energy of the body, and on the other the energy of the outer world, can be diminished or augmented.

Therefore the energy of the body is also subject to a continual variation, a continual fluctuation; it increases and diminishes, even though the volume and temperature are assumed to be constant. If, therefore, it is said the energy of a body is constant, this properly means only that the mean value during

an oscillation

$$\bar{E} = \text{const.}$$

The energy of a body consists of two parts—the *vis viva*  $T$ , and the potential energy  $U$ ; so that

$$E = T + U.$$

Now since  $E$  is subject to continual alterations,  $T$  and  $U$  will in general continuously vary; their mean value, however, for a complete period shall remain constant. Thus

$$\bar{T} = \text{const.},$$

$$\bar{U} = \text{const.},$$

and

$$\bar{E} = \bar{T} + \bar{U}.$$

The energy,  $dQ$ , received from without in the time  $dt$  is, according to the principle of the conservation of energy,

$$dQ = dT + dU + dW.$$

Let us select any one of the material points of the body as the representant of the others. Let  $m$  be the mass of this point; let its rectangular coordinates, at the time  $t$ , be  $x, y, z$ ; the components of its velocity  $x', y', z'$ ; and the components of its acceleration  $x'', y'', z''$ . In the time  $dt$  the coordinates and the components of the velocity will vary with  $dx \dots$  or with  $dx' \dots$ . And since

$$T = \frac{1}{2} \Sigma m(x'^2 + y'^2 + z'^2),$$

and the total work (external and internal) performed in the time  $dt$ ,

$$dU + dW = -\Sigma m(x''dx + y''dy + z''dz)$$

(if the work which the forces do against the body be reckoned positive).

Therefore

$$dQ = \Sigma m(x'dx' + \dots - x''dx - \dots),$$

or

$$dQ = \Sigma m \left( \frac{dx}{dt} dx' \dots - \frac{dx'}{dt} dx \dots \right);$$

hence

$$dQ = 0.$$

From this it follows that *this spontaneous periodical change of the body is an adiabatic one*; the energy which comes into play in this alternate performance and consumption of work is exclusively borrowed from the energy of the body.

The equation of energy for the unchanged state of the body is therefore to be written as follows,

$$0 = dT + dU + dW.$$

Then, taking the mean values,

$$d\bar{T}=0, \quad d\bar{U}=0, \quad d\bar{W}=0.$$

Let us collect the results of the foregoing considerations. It is now easy to perceive when the thermal state of a body can be regarded as constant: it is not in the least necessary that what are called the variables of state (volume, temperature, tension, &c.) should continually remain constant. According to our apprehension, the condition for the constancy of the state of any body whatsoever consists in this, *that the period of the oscillation, the mean values of the volume, of the vis viva, and of the potential energy be constant, the mean value of the external work, referred to the duration of a complete oscillation, be nil, and, finally, that the periodical change of the body be an adiabatic one.* Therefore, if the state is unchanged, then

$$\bar{i} = \text{const.}, \quad . \quad . \quad (1) \quad \bar{U} = \text{const.}, \quad . \quad . \quad (4)$$

$$\bar{v} = \text{const.}, \quad . \quad . \quad (2) \quad d\bar{W} = 0, \quad . \quad . \quad (5)$$

$$\bar{T} = \text{const.}, \quad . \quad . \quad (3) \quad d\bar{Q} = 0. \quad . \quad . \quad (6)$$

So long as the state of the body is unchanged these six equations stand, and *vice versâ*. But these condition-equations are not all independent of one another; it is possible that by satisfying two or three the rest of them are *eo ipso* satisfied.

Having settled these preliminaries, we will now pass to the analytic treatment of state-change, and, in connexion therewith, to the deduction of the Second Proposition.

## 2.

Let us imagine any body, and select any one of its material points. Let  $m$  be the mass of this point; let its rectangular coordinates at the time  $t=0$  be  $x_0, y_0, z_0$ ; the components of its velocity,  $x'_0, y'_0, z'_0$ ; and the components of its acceleration,  $x''_0, y''_0, z''_0$ . Further, let  $v_0$  be the volume of the body,  $T_0$  its *vis viva*,  $U_0$  its potential energy at the same time. If the state of the body did not vary, the period of an oscillation would remain constant, likewise the mean values referred to it  $\bar{v}$ ,  $\bar{T}$ ,  $\bar{U}$ , &c., and the point  $m$  would come at the end of the time  $i$  into a certain position whose coordinates we will designate by  $x_1, . . . .$ , the velocity-components there appearing by  $x'_1, . . . .$ , the acceleration-components by  $x''_1, . . . .$ . But now let the state of the body change infinitesimally during the time  $i$ , so that at the end of it the oscillation-period may be  $i + \delta i$ , the mean value of the *vis viva*  $\bar{T} + \delta \bar{T}$  (instead of  $\bar{T}$ ), &c.

At any instant  $t$ , between 0 and  $i$ , let the coordinates of  $m$  be  $x, . . . .$ , the velocity-components  $x', . . . .$ , and the



acceleration-components  $x'', \dots$ ; at the same time let the *vis viva* of the body be  $T$ , its potential energy  $U$ . In the indefinitely short time  $dt$  subsequent to  $t$  the position of the point is changed by  $\frac{\delta x \cdot dt}{i}, \dots$ , its velocity by  $\frac{\delta x' \cdot dt}{i}, \dots$ , the *vis viva* of the body by  $\frac{\delta T \cdot dt}{i}$ , its potential energy by  $\frac{\delta U \cdot dt}{i}$ . Meanwhile the body does the work  $\frac{\delta W \cdot dt}{i}$ , and receives from without the energy  $\frac{\delta Q \cdot dt}{i}$ . According to this notation, the variation  $\delta$  signifies that indefinitely small change of the quantity concerned which would ensue during the time  $i$  if the rate of the change remained the same throughout the time  $i$  as it was during the element of time  $dt$ . From the principle of the conservation of energy we have then

$$\delta Q = \delta T + \delta U + \delta W.$$

Since

$$T = \Sigma \frac{m}{2} (x'^2 + y'^2 + z'^2)$$

and

$$\delta U + \delta W = -\Sigma (x'' \delta x + y'' \delta y + z'' \delta z)$$

(that is, when the work is reckoned positive which the forces do *against* the body), therefore

$$\delta Q = \Sigma m (x' \delta x' + \dots - x'' \delta x - \dots).$$

Now, in order that we may form  $\overline{\delta Q}$ , we multiply the equation by  $dt$  and take into consideration that

$$\begin{aligned} x' dt &= dx, \\ x'' dt &= dx'. \end{aligned}$$

Then

$$\delta Q \cdot dt = \Sigma m (dx \cdot \delta x' + \dots - dx' \delta x - \dots).$$

But now

$$dx' \delta x = d(x' \delta x) - x' d\delta x$$

and

$$x' d\delta x = x' \delta dx = \delta(x' dx) - dx \delta x';$$

so that

$$\delta Q \cdot dt = -\Sigma m d(x' \delta x + \dots) + \Sigma m \delta(x' dx + \dots),$$

or, writing the symbols of differentiation and variation before that of summation, and restoring in the last sum the previous value of  $dx$ , taking into account the expression of the *vis viva*, and integrating between 0 and  $i$ ,

$$\int_0^i \delta Q \cdot dt = \Sigma m [x' \delta x + \dots]_i^0 + \int_0^i 2T \cdot dt,$$

or, inserting the mean values,

$$i \cdot \delta \bar{Q} = \Sigma m [x' \delta x + y' \delta y + z' \delta z]_i^0 + \delta(2 \cdot i \bar{T}). \quad \dots \quad (\text{I.})$$

This equation is, as remarked by Sir William Thomson in 'A Treatise on Natural Philosophy,' i. p. 233, *valid for every system under all circumstances.*

Let us now investigate the signification of the here occurring quantities.

$\delta \bar{Q}$  is the integral of  $\frac{\delta Q \cdot dt}{i}$  between the limits 0 and  $i$ ; and since  $\frac{\delta Q \cdot dt}{i}$  denotes the energy which the body receives from without during the time  $dt$ , its integral will denote the total energy which it receives from without during the time of a pulsation, viz. the energy imparted at the infinitely small variation of state.

The first sum on the right side of the equation,

$$\Sigma m(x'_0 \delta x_0 + \dots),$$

refers to  $t=0$ —that is, to the *first* time-element of the state-variation; the second sum,

$$\Sigma m(x'_i \delta x_i + \dots),$$

refers to  $t=i$ —that is, to the *last* time-element of the same. Between these two instants lies just one period of the unchanged body; hence, as long as the state of the body does not change, its phases at the instants 0 and  $i$  are the same; consequently

$$v_i = v_0, \quad T_i = T_0, \quad U_i = U_0,$$

and

$$t_i = t_0 + i;$$

therefore

$$dt_i = dt_0, \quad \delta T_i = \delta T_0, \quad \delta U_i = \delta U_0.$$

Further, the external works performed in the time-elements  $dt_i$  and  $dt_0$  (the same infinitely small variation of state starting from the same phase) will be likewise the same; that is to say,

$$\frac{\delta W_i dt_i}{i} = \frac{\delta W_0 dt_0}{i},$$

and hence

$$\delta W_i = \delta W_0.$$

Premising these, let us see the dynamical signification of the sum

$$\Sigma m(x' \delta x + \dots).$$

To this end let us consider first the signification of

$$\Sigma m(x'dx + \dots).$$

This may be written also as follows,

$$\Sigma m(x'^2 + \dots) \cdot dt,$$

or thus,

$$2T \cdot dt.$$

But this last expression is nothing else but the *action* of the unchanged body in the time  $dt$ . Let that action be  $dA$ , then

$$\Sigma m(x'dx + \dots) = dA,$$

whence it appears that the sum in question, viz.

$$\Sigma m(x'\delta x + \dots) = \delta A,$$

signifies the action of the body in the time  $i$ , supposing that its material points are not endued with any spontaneous motion, and that the rate of change of state remains the same throughout all the time  $i$  as it actually was during the element of time  $dt$ . Hence

$$\frac{\delta A \cdot dt}{i}$$

denotes the action of the body in the time  $dt$ , supposing that its material points are not endued with any spontaneous motion. If we now add to this the action in the time  $dt$  resulting from the spontaneous motion of the material points, we shall get the total action in the time  $dt$  equal to

$$\frac{\delta A \cdot dt}{i} + dA.$$

Accordingly

$$\frac{\delta A_0 \cdot dt_0}{i} + dA_0$$

will express the action in the first time-element of the change of state.

Let us now imagine that the very same infinitely small state-variation does not begin at the time  $t=0$ , but at the time  $t=i$  later, and that it also finishes at the same time later; then will the action in the first time-element of the state-variation be

$$\frac{\delta A_i \cdot dt_i}{i} + dA_i.$$

But according to the above-defined conceptions, the thermal properties of the body are periodical functions of the time; consequently it cannot make any difference whether the change of state starts with the beginning of the  $n$ th or  $(n+1)$ th period; and the action in the first time-element of the change must be

the same, in whichever period the change happen, provided only the phase be the same. Hence

$$\frac{\delta A_i \cdot dt_i}{i} + dA_i = \frac{\delta A_0 \cdot dt_0}{i} + dA_0,$$

or

$$\frac{\delta A_i \cdot dt_i}{i} + 2T_i \cdot dt_i = \frac{\delta A_0 \cdot dt_0}{i} + 2T_0 \cdot dt_0;$$

and since

$$T_0 = T_i, \text{ and } dt_0 = dt_i,$$

we shall have

$$\delta A_i = \delta A_0;$$

and considering the signification of  $\delta A$ ,

$$\Sigma m(x'_i \delta x_i + \dots) = \Sigma m(x'_0 \delta x_0 + \dots),$$

or

$$\Sigma_i = m[x' \delta x + y' \delta y + z' \delta z]_i = 0.$$

We arrive therefore at the result that

$$i \overline{\delta Q} = \delta(2i\overline{T}),$$

or, in words, *The variation of the action of the body during a pulsation is equal to the product of the period of the pulsation and the quantity of energy communicated to it during that time.*

As in this equation the variation-symbols only are preserved, we can substitute differentiation-symbols for them, and write

$$i \cdot \overline{dQ} = d(2i\overline{T}). \quad . \quad . \quad . \quad . \quad . \quad (I.A)$$

*If constancy of state be defined in the way in which we have defined it, equation (I.A) will be valid for every body and for any infinitesimal change of state.*

If the body goes through a closed series of state-changes (a "cyclical process"), so that  $i$  and  $T$  at the close resume the initial values, then is

$$\oint i \cdot \overline{dQ} = 0,$$

or

$$\iint dQ \cdot dt = 0,$$

where the first integral refers to a complete oscillation, and the second to an entire cyclical process.

Equation (I.A), however, can also be integrated in another way. If we divide it by  $i \cdot \overline{T}$ , we get

$$\frac{\overline{dQ}}{\overline{T}} = 2 \cdot d \log(i\overline{T}),$$

or, otherwise written,

$$\frac{\int_0^i dQ \cdot dt}{\int_0^i T dt} = 2 \cdot d \log \left( \int_0^i T dt \right).$$

In the first form, integrated over an entire cyclical process, it is

$$\int \frac{dQ}{T} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (II. B)$$

3.

If we now compare these dynamic theorems with the second proposition of the mechanical theory of heat, which is referred to reversible cyclical processes, we find

$$\int \frac{dQ}{a+t} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

where  $a+t$  expresses the temperature reckoned from absolute zero of gases; while of  $dQ$  it is asserted that it denotes the differential of the heat communicated to the body. Our dynamic equation (II. B) indeed agrees with the thermodynamic equation (II.), but only on the hypothesis that the absolute temperature is proportional to the mean value of the *vis viva* of the body, referred to the duration of an oscillation, and that the thermodynamic  $dQ$  is proportional to the dynamic  $\overline{dQ}$ . We will first examine the latter relation, designating, for the sake of distinction, the thermodynamic  $dQ$  by  $d\mathfrak{Q}$ , so that

$$d\mathfrak{Q} = \alpha \overline{dQ}.$$

And since both  $d\mathfrak{Q}$  (heat) and  $\overline{dQ}$  denote energy, the proportionality-factor  $\alpha$  is a pure number, the quantity of which depends only on the units chosen. Selecting the unit of heat equal to the unit of energy, we get  $\alpha=1$  and

$$d\mathfrak{Q} = \overline{dQ},$$

or

$$d\mathfrak{Q} = \frac{1}{i} \int_0^i dQ \cdot dt.$$

From this it follows that what is called the heat-differential,  $d\mathfrak{Q}$ , is not the differential of the energy, but denotes only the mean value of such differentials during a complete oscillation. One can now easily explain why the first principal equation of the theory of heat cannot be directly integrated. It is because  $d\mathfrak{Q}$  is not properly a differential, but only an infinitesimal average quantity, and because the sum of the average quantities

$$\int \frac{1}{i} \int_0^i dQ \cdot dt$$

depends also on the temporary course (in other words, on the way and manner) of the importation of energy. Further, up to the present the period has been quite undetermined during which the quantity of heat  $dQ$  is communicated to the body; it is now evident that  $dQ$  refers to the period of an oscillation, and that it signifies that quantity of energy which should be brought to the body during one oscillation.

Let us now examine the relation of the absolute temperature to the *vis viva*. Provisionally let  $\beta$  be the proportionality-factor, so that

$$\bar{T} = \beta(a + t).$$

As the temperature is independent of the mass (for a small mass may have the same temperature as a large one), while  $\bar{T}$  depends on the mass, it is simplest to suppose  $\beta$  proportional to the mass of the body. Thus

$$\beta = M \cdot c,$$

where  $M$  denotes the mass of the body, and  $c$  a pure number which, beside the chosen units of measurement, can only depend on the nature of the body. We then have

$$a + t = \frac{\bar{T}}{M \cdot c}; \quad . \quad . \quad . \quad . \quad . \quad (7)$$

that is, *the absolute temperature of the body is to the mean value of the vis viva in direct, and to the mass in inverse ratio.* The measuring unit is the *vis viva* divided by the mass, or (what is the same thing) the square of the unit of length divided by the square of the unit of time.

Let us moreover determine what is signified by the number  $c$ . Let us imagine the entire heat communicated applied only to the augmentation of the mean value of the *vis viva*, and therefore

$$d\bar{Q} = d\bar{T},$$

and consequently, according to equation (7),

$$d\bar{Q} = M c \cdot dt.$$

From this it follows that  $c$  denotes the *true heat-capacity*—that is, the number that states how much heat the unit of mass requires in order that the temperature may be raised one degree.

As an example, the application of equation (8) to the permanent gases may serve. In these, according to the experiments of Joule and Thomson, the specific heat at constant volume differs but very little from the true heat-capacity. Therefore, if  $\bar{V}$  is the volume of the body,  $\bar{v}$  its specific volume,  $c$  the specific heat at constant volume referred to the unit of

mass, and  $\gamma$  the specific heat at constant volume referred to the unit of volume, we have

$$\bar{v} = \frac{\bar{V}}{\bar{M}},$$

and

$$\gamma = \frac{c}{v};$$

hence

$$\bar{M} \cdot c = \bar{V} \cdot \gamma,$$

and

$$a + t = \frac{\bar{T}}{\bar{V} \cdot \gamma}.$$

Hydrogen, oxygen, nitrogen, atmospheric air, and carbonic oxide are permanent gases; and their molecules contain the same number of atoms. Now, as it follows from Regnault's experiments that in these  $\gamma$ , independent of the nature of the gas, is very nearly equal, therefore for the gases mentioned

$$\frac{a+t}{a+t_1} = \frac{\bar{T}}{\bar{V}} \cdot \frac{\bar{V}_1}{\bar{T}_1}.$$

Consequently, when it is said that *the temperature of two gases (whose molecules contain the same number of atoms) is equal, this much is meant—that the mean value of the vis viva for the unit of volume, in the course of a complete oscillation, is equal in the two gases.*

Having thus fixed the dynamic signification of the quantities which occur in the Second Proposition, let us return to the equation

$$\bar{dQ} = \frac{2d(i\bar{T})}{i}.$$

If we divide it by  $2\bar{T}$ , we get

$$d \log (i\bar{T}) = \frac{\bar{dQ}}{2\bar{T}};$$

and since the left side is a differential, the right side must also be a differential. If we designate it simply by  $dS$ , then  $S$  will signify the half of the "Entropy:"

$$d \log (i\bar{T}) = dS.$$

Integrating this equation, and designating the integration-constant by  $\log C_1$ ,

$$i = C_1 \cdot \frac{e^S}{\bar{T}}.$$

Were the function  $S$  and the constant  $C_1$  known, the duration of an oscillation could be calculated from this formula. In the case of the permanent gases we can readily determine  $S$ ; for we know that with these

$$d\overline{Q} = M \cdot c_v \cdot dt + M(c_p - c_v)(a + t) \frac{dv}{v},$$

and, designating the ratio of the two specific heats by  $k$ ,

$$d\overline{Q} = d\overline{T} + (k-1)\overline{T} \frac{dv}{v};$$

therefore

$$\frac{d\overline{Q}}{2\overline{T}} = dS = \frac{1}{2} d \log (\overline{T} v^{k-1}).$$

Hence, for permanent gases,

$$e^S = C_2 \overline{T}^{\frac{k-1}{2}} v^{\frac{k-1}{2}},$$

where  $C_2$  denotes the constant occurring in this integration. Substituting this value of  $S$  in the general formula of the oscillation-period, and putting  $C$  in the place of the product of the constants  $C_1$  and  $C_2$ , we get

$$i = C \sqrt{\frac{v^{k-1}}{\overline{T}}}.$$

Let us now consider two gases whose molecules contain the same number of atoms, at equal tension and equal temperature. Let  $\rho$  and  $\rho_1$  be their densities under normal circumstances,  $i$  and  $i_1$  their oscillation-periods; then is

$$i = i_1 \sqrt{\left(\frac{\rho_1}{\rho}\right)^{k-1}}.$$

If we refer the density to that of hydrogen as unit and take the oscillation-period of hydrogen as the time-unit, then is

$$\frac{\rho}{\rho_1} = d,$$

and

$$i = d^{-\frac{k-1}{2}}.$$

With gases whose molecules consist of two atoms each,

$$k = 1.405, \quad \frac{k-1}{2} = 0.2025;$$

therefore

$$\log i = -0.2025 \log d.$$



On the basis of this equation the following Table was calculated :—

Name of the gas.	Density.	Oscillation-period.
Hydrogen . . . .	1	1
Oxygen . . . .	16	0.5704
Nitrogen . . . .	14	0.5860
Atmospheric air . .	14.438	0.5825

From this Table it is seen that the denser gas pulsates more quickly, the less dense more slowly; the duration of an oscillation is nearly twice as great for hydrogen as for oxygen.

Only one brief remark more. If it is true, as we have reasons for assuming, that in nature, on account of imperfect elasticity, friction, viscosity, and the radiation of energy, reversible cyclical processes cannot actually take place, that consequently

$$\int \frac{dQ}{(a+t)}$$

is ever greater than *nil*, then must also

$$\int id\bar{Q}$$

be always greater than *nil*. In other words, the function

$$A = 2i\bar{T},$$

which Maupertius named "action," is (with respect to the source of the energy received) absolutely *less* in amount at the end of each variation than at its commencement. Hence the hypothetic proposition, that the entropy of the universe is always increasing (CLAUSIUS), can also be expressed thus :—  
*The action of the universe is continually diminishing.*

XXXIV. *On Picoline and its Derivatives.* By WILLIAM RAMSAY, *Ph.D.*, *Tutorial Assistant of Chemistry in the Glasgow University*\*.

**B**ASES of the pyridine series have been formed in a number of different ways. Perkin (*Journ. Chem. Soc.* [2] vol. iii. p. 173) prepared pyridine by reducing azodinaphthyl-diamine with tin and hydrochloric acid. It was found as a constant product along with naphthendiamine and naphthylamine, but only in small quantity. As pyridine has a formula exactly half of that of naphthendiamine,  $C_{10}H_{10}N_2$ , it is possible that the latter splits up into two molecules of pyridine. But Perkin was unsuccessful in preparing naphthendiamine from pyridine.

\* Communicated by the Author.



found that when the latter acid is decomposed into pyridene, nitrogen is evolved, and not carbonic anhydride.

Dewar suggested that pyridine might possibly be obtained by the mutual action of acetylene and hydrocyanic acid on each other at a high temperature. The equation by which the reaction is expressed is this:—



A gas-holder of acetylene was prepared by burning air in coal-gas; and from this acetylene the silver compound was formed, so as to ensure its purity. The silver compound was decomposed with hydrochloric acid, and the acetylene, after being washed, was passed through strong hydrocyanic acid, and then through a bright-red-hot iron tube, the other end of which communicated with a Liebig's condenser. Only about 2 cubic centimetres of distillate were obtained. Hydrochloric acid was added, and the liquid was evaporated to dryness on a water-bath; benzol was apparently evolved, as far as could be judged by the smell. Sodium hydrate was added to the residue, when a few drops of an oily liquid separated smelling strongly of pyridine; unfortunately too little was obtained to admit of confirmation; but the extremely characteristic smell of the base leaves no doubt that one of the series was actually formed. I propose to repeat this experiment on a larger scale.

### *Salts of Picoline.*

*Picoline hydrochloride*, described by Anderson, is a white extremely deliquescent crystalline salt, and melts at  $160^\circ$ . It is best prepared by neutralizing picoline with hydrochloric acid, and distillation, after evaporation to dryness.

*Picoline hydrobromide* may be obtained in the same manner as the hydrochloride, but is more easily prepared by cautiously adding bromine to picoline and distilling the resulting oil. I shall consider the action of bromine on picoline in a subsequent part of this memoir. *Picoline hydrobromide* melts at  $187^\circ$ ; and its boiling-point is considerably above that of the chloride, and also above the range of a mercurial thermometer. It is a white crystalline mass, and also deliquesces, but not so readily as the chloride. Both the chloride and bromide may be obtained in feathery crystals by using impure picoline as a solvent.

*Picoline hydriodide* cannot be prepared by distilling a mixture of the base with hydriodic acid; for picoline is liberated, and the hydriodic acid decomposes, yielding a compound which will be afterwards described.

*Action of Chlorine on Picoline Hydrochloride.*—Picoline

hydrochloride appears to absorb chlorine when a current of that gas is passed over it, but evolves the chlorine on exposure to air. No apparent change is produced even when the hydrochloride is strongly heated during its treatment with chlorine.

*Action of Bromine on Picoline Hydrobromide.*—The result of treating the hydrobromide with bromine is different. When bromine is added to a solution of the hydrobromide in water, a mass of needles is formed almost instantaneously. The reaction takes place easily if a strong solution of picoline hydrobromide be heated with bromine in a sealed tube to  $190^{\circ}$  for an hour. The yellowish-red oil which at first sinks to the bottom, dissolves in the water; and on cooling, the fluid is filled with beautiful golden scales, while a reddish-yellow crystalline mass remains at the bottom of the tube, consisting of excess of the compound with bromine which the water was not able to dissolve. The scales, after being collected and dried over sulphuric acid, had a distinct smell of bromine. On analysis the following numbers were obtained:—

I. 0.3015 grm. of substance gave 0.5043 grm. of  $\text{AgBr}$  = 71.17 per cent. bromine.

II. 0.3683 grm. of substance gave 0.5725 grm. of  $\text{AgBr}$ , and 0.0088 grm. of  $\text{Ag}$ , together equal to 68.04 per cent. bromine.

The former analysis was performed with freshly prepared material; while the substance analyzed giving the results of No. II. had stood over sulphuric acid for some days, and had evidently lost bromine. The result of No. I. agrees sufficiently well with the formula  $\text{C}_8\text{H}_7\text{N} \cdot \text{Br}_2 \cdot \text{HBr}$ , which demands 71.85 per cent. of bromine.

When the dibromide of picoline hydrobromide is heated with water in an open vessel, bromine is evolved; and it is possible thus to remove the two additive atoms of bromine, leaving pure picoline hydrobromide. It is sparingly soluble in water; and if soda or potash is added to its aqueous solution, picoline is liberated; if the alkaline liquid is acidified, the bromine compound appears to be again formed. It melts at about  $85^{\circ}$ ; but as it evolves bromine when warmed, the exact point is not easily determined.

Bromine does not act substitutingly on picoline hydrobromide; for even after heating the two together for a week to  $180^{\circ}$ , no hydrobromic acid was evolved, although a black tarry mass was formed.

Nicotine forms a similar additive compound with bromine; and this constitutes an additional proof that there is some connexion between the two bases, nicotine and picoline or pyridine.

*Action of Iodine on Picoline Hydriodide.*—When picoline

is mixed with strong aqueous hydriodic acid, and distilled after excess of water has been removed by evaporation, the temperature rises rapidly, and the mass turns brown and viscid. If the distillation be continued, picoline distils over, and the residue, when cool, solidifies to a mass of reddish-brown needles. These needles dissolve in alcohol and in ether, but cannot be recrystallized from either of these solvents; they are insoluble in carbon disulphide. When brought into contact with water, they melt at once to a deep brown liquid. On analysis this substance was found to have the formula  $C_6H_7N \cdot I_2 \cdot HI$ , which requires 80.21 per cent. I.

0.4300 grm. gave 0.6377 grm.  $AgI = 80.14$  per cent. I.

The crystals were analyzed by dissolving them in alcohol and converting the additive iodine into hydriodic acid by sulphurous anhydride. The melting-point of the diiodide of picoline hydriodide is  $79^\circ$ . Before the method of preparation which has just been described had been discovered, an attempt had been made to prepare this substance (which is analogous to the bromine compound) by heating picoline with hydriodic acid and iodine in a sealed tube; but no signs of crystallization appeared, even when the resulting dark brown liquid was cooled to  $0^\circ$ . As the compound turns liquid when brought into contact with water, this behaviour is explained.

*Action of Chlorine on Picoline.*—When chlorine is passed into picoline, the phenomena described by Anderson are seen. The picoline becomes filled with a white flocculent precipitate; and soon a violent action ensues with great evolution of heat, and the whole mass is transformed into a brown resin which does not admit of purification. If, however, the picoline be poured, in small quantity at a time, into a capacious flask filled with dry chlorine and kept cool, the formation of the resin is avoided, and an oily liquid is formed, which partially crystallizes on cooling; on addition of water it is changed to a white powder. This powder was collected on a filter, washed with cold water, in which it is very slightly soluble, and dried over sulphuric acid. It agreed in properties with the analogous compound obtained from pyridine by Anderson. It has a smell closely resembling that of bleaching-powder, and turned brown on application of heat. Anderson believed it to be the hydrochloride of trichloropicoline,  $C_6H_4Cl_3N \cdot HCl$ , and apparently obtained concordant numbers on analysis. He states that it turned brown when heated to  $100^\circ$ , and lost hydrochloric acid, leaving trichloropicoline.

This substance was analyzed by combustion with lead chromate, as the formula given by Anderson did not appear satisfactory.

I. 0.3915 grm. of substance gave 0.3696 grm.  $\text{CO}_2$  and 0.1006 grm.  $\text{H}_2\text{O}$ .

II. 0.2991 grm. of substance gave 0.2782 grm. of  $\text{CO}_2$  and 0.0762 grm. of  $\text{H}_2\text{O}$ .

III. 0.2900 grm. of substance gave 0.2801 grm. of  $\text{CO}_2$  and 0.0798 grm. of  $\text{H}_2\text{O}$ .

IV. 0.3489 grm. of substance gave, on combustion with soda-lime and ignition of the platinum-salt, 0.1382 grm. of Pt.

V. 0.2676 grm. of substance gave 0.5327 grm. of  $\text{AgCl}$ .

VI. 0.3230 grm. of substance gave 0.6410 grm. of  $\text{AgCl}$ .

	I.	II.	III.	IV.	V.	VI.
C .	25.74	25.36	26.34			
H .	2.85	2.83	3.05			
N .	.....	.....	.....	5.63		
Cl .	.....	.....	.....	.....	49.25	49.09
	Mean.	Calculated for $\text{C}_6\text{H}_4\text{Cl}_3\text{N} \cdot \text{HCl}$		Calculated for $\text{C}_6\text{H}_6(\text{OCl})_3\text{N} \cdot \text{HCl}$		
C .	25.81	30.90		25.44		
H .	2.91	2.14		2.47		
N .	5.63	6.01		4.94		
Cl .	49.17	60.94		50.17		
	83.52	99.99				
O, by difference	16.48			16.96		
	100.00			99.98		

Another chlorine-estimation gave 50.57 per cent. of chlorine ; a different preparation was used. Taking the number of carbon atoms as 6, the numbers found stand to each other in the ratio of  $\text{C}_6\text{H}_{8.1}\text{N}_{1.1}\text{Cl}_{3.85}\text{O}_{2.87}$ . As the substance smells like hypochlorous acid, and as it is evidently produced by the action of water on the crystals or on the oil, which are the first products of the action of chlorine on picoline, it may possibly contain the group  $(\text{OCl})$ , and might have the formula  $\text{C}_6\text{H}_6(\text{OCl})_3\text{N} \cdot \text{HCl}$ . A gas is evolved in small quantity when the oil is brought into contact with water, which appears to be chlorine. The numbers do not agree at all well ; and I should have investigated the compound more minutely had it not been for the great waste of picoline in its preparation. From 0.5 grm. of picoline 0.041 grm. of the white compound was obtained ; and the filtrate, which contained chiefly picoline hydrochloride, yielded on evaporation and precipitation with platinum chloride 0.7582 grm. of impure picoline platinochloride, corresponding to 0.3290 grm. of the hydrochloride. I believe that a more complicated action takes place when water is added to the product of the action of chlorine on picoline than a mere precipitation of the white powder. The white powder is

sparingly soluble in water, and communicates to it the taste of a hypochlorite; it dissolves in alcohol, and in glacial acetic acid, and is precipitated on addition of water. When distilled with soda-lime, ammonia and hydrogen are evolved, and no carbonaceous residue is left in the tube. As the chlorine compound evolved a peculiar smell when distilled with water, an attempt was made to obtain some decomposition-products which might lead to a better knowledge of its constitution. Ether extracted from the distillate a few oily drops having a very pungent smell; by far the greater part of the chlorine compound turned to a resin, which yielded no promising results on attempted purification. This oil also is formed when the chlorine compound is distilled with soda, but not apparently in greater quantity. The chlorine compound is reconverted into picoline hydrochloride by treatment with tin and hydrochloric acid.

*Action of Bromine on Picoline.*—When bromine is added to picoline or when picoline is exposed to the action of bromine vapour, it is transformed into a dark brown resinous-looking substance. If this product is distilled, pure picoline hydrobromide comes over. The products formed are probably picoline hydrobromide, and an analogous compound to that formed by the action of chlorine on picoline; more bromine adds on to the hydrobromide and forms a dibromide of that salt; and direct union also probably takes place between the picoline and bromine, forming a compound which was obtained in a pure state by treating picoline with a solution of bromine in chloroform; the chloroform was removed by evaporation *in vacuo*. A substance which crystallized in small interlaced needles was deposited. It had a faint smell of bromine, and on analysis was found to have the formula  $C_6H_7N.Br_2$ .

0.2535 grm. of substance gave 0.3523 grm. of Ag Br and 0.0048 grm. of Ag, together equal to 0.1534 grm. of Br, or 60.51 per cent. Theory demands 63.24 per cent.; but bromine was evidently lost on exposure to air.

A similar compound was formed by acting on picoline with a solution of iodine monochloride in chloroform; the liquid grew warm, and a direct addition-product was formed. On evaporation of the chloroform in a vacuum in presence of caustic soda and sulphuric acid to prevent moisture, and to absorb chlorine, long yellow needles were deposited. An estimation of chlorine and iodine gave the following results:—

0.3044 grm. of substance gave 0.4488 grm. of Ag Cl + Ag I, which on reduction yielded 0.2565 grm. of Ag.

I = 49.83 per cent. Cl = 13.63 per cent. Theory for  $C_6H_7N.ICl$ , I = 49.70 per cent. Cl = 13.89 per cent.

There can be no doubt that these bodies are direct addition-products ; for, besides the fact of their evolving picoline when treated with potash, the bromine compound when heated decomposes violently, and changes to a resin. This would not have happened had the bromine acted on picoline hydrobromide and not on picoline.

Such, then, are the results of the action of the halogens on picoline. They appear to form four distinct products :—(1) a direct addition compound containing picoline plus two atoms of halogen ; (2) a substitution compound which undergoes alteration when brought into contact with water ; (3) a salt of the halogen acid ; and (4) an addition-product containing two atoms of the halogen combined with the haloid salt. Most probably, when chlorine or bromine acts on picoline, all four are formed, besides a resin resulting from some deeper action. If the resin be warmed in an atmosphere of chlorine, it becomes lighter in colour, refuses to dissolve in water, and smells strongly of chlorine ; I have not, however, examined the product.

I shall now describe a few of the salts of picoline which have not previously been prepared.

*Ferrocyanide*.—Picoline, treated with an aqueous solution of hydroferrocyanic acid, deposited after some weeks crystals of a compound which was probably the ferrocyanide ; but a large amount of a blue precipitate was also formed.

*Platinocyanide*.—This beautiful salt was prepared by treating picoline with the calculated amount of hydroplatinocyanic acid, and evaporating the solution over sulphuric acid. It crystallizes in pale yellow transparent crystals, apparently rhomboidal. It was analyzed with the following result :—

0.5028 at  $120^{\circ}$  lost 0.0628 = 12.50 per cent.  $\text{H}_2\text{O}$ , and on ignition gave 0.1770 grm. of Pt = 35.20 per cent.

Calculated for  $\text{Pt}(\text{CN})_2 \cdot 2\text{HCN} \cdot 2\text{C}_6\text{H}_7\text{N} \cdot 4\text{H}_2\text{O}$ ,

$\text{H}_2\text{O}$  = 12.83 per cent., Pt = 35.11 per cent.

On recrystallization from a hot solution, needles were deposited which appear to contain a different amount of water of crystallization ; the platinum found (34.3 per cent.) corresponds to a salt containing 5  $\text{H}_2\text{O}$ , which demands 34.02 per cent. of Pt.

1 part of picoline platinocyanide dissolves in 83 parts of water at  $10^{\circ}$ .

*Tartrate*.—Two molecules of picoline were mixed with one molecule of tartaric acid, and the solution was allowed to evaporate over sulphuric acid ; after a week, long white needles were formed, which smelled faintly of picoline. They



were analyzed by distillation with a solution of sodium hydrate, the picoline being received in hydrochloric acid and estimated as platinochloride.

0.5500 grm. gave 0.314 grm. of platinum = 0.3015 grm. of picoline, or 54.81 per cent. The formula  $C_4 H_6 O_6 \cdot 2 C_6 H_7 N$  requires 55.35 per cent.

With *citric acid* picoline forms an uncrystallizable syrup. It refuses to combine with *acetic* or *formic acid*; and an attempt to prepare a *chromate* was also unsuccessful. The *phosphate* is a crystalline mass which rapidly absorbs water. The *chlorate* formed diamond-shaped crystals of extreme thinness. It explodes when heated; it was prepared by neutralizing the free acid with picoline, and is sparingly soluble in water.

*Picoline mercuric chloride*,  $C_6 H_7 N \cdot Hg Cl_2$ , is precipitated as a white powder by adding picoline to a solution of mercuric chloride. It is sparingly soluble in cold, but dissolves more easily in hot water, from which it crystallizes in needles. It is more soluble in alcohol, and crystallizes from that solvent in plates. On analysis 75.6 and 76.1 per cent. of mercury were found instead of 74.27, demanded by theory. This compound is not acted on by chlorine.

*Compounds of Picoline with the Haloid Compounds of Alcohol and Acid Radicals.*

1. *With Alcohol Radicals. Picoline Methyl Salts.*—Equal volumes of picoline and methyl iodide were mixed in a tube, which was tightly corked and allowed to stand for some time. The mixture became turbid and grew very warm, and partially solidified. As soon as the tube was cold, the excess of methyl iodide was distilled off, and the solid residue dried between folds of bibulous paper and recrystallized from alcohol, in which it is moderately soluble. It crystallized in long white needles. Picoline methyl iodide is slightly deliquescent, and melts at 226.5–227° (uncorr.).

The methyl chloride was prepared from the methyl iodide by boiling an aqueous solution of the latter with silver chloride. On evaporation on the water-bath it formed a syrup, which soon solidified to a crystalline mass. It is extremely deliquescent, and becomes moist after exposure to air for a few seconds. It crystallizes from alcohol in needles. The platinum salt crystallizes in small cubes.

0.1583 grm. of the platinum salt gave 0.0481 grm. of platinum = 30.38 per cent. of Pt.

Theory for  $(C_6 H_7 N \cdot CH_3 Cl)_2 Pt Cl_4$ , 30.33 per cent.

The *methyl nitrate*, prepared from the methyl iodide by addition of silver nitrate, crystallizes in long transparent prisms,

which may easily be obtained some inches long. It is slightly deliquescent, and deflagrates when heated, leaving a bulky residue of charcoal, and evolving a smell of dipyridine compounds.

The *methyl sulphate* crystallizes with difficulty.

The *methyl hydrate*, prepared from the methyl iodide with moist silver oxide, is colourless at first, but soon turns greenish red. Its properties are analogous to those of the ethyl hydrate described by Anderson. It is decolorized by chlorine; but no definite compound could be obtained. Anderson stated that the hydrate of ethyl picoline, when boiled, evolved ethylamine; the distillate from an aqueous solution of about 5 grms. of the methyl hydrate had a peculiar smell resembling leeks, but yielded an unweighably small amount of platinum salt; so that no reliable results could be obtained on igniting it. The residue from the distillation was evaporated to dryness on the water-bath, and assumed a bright red colour when dry. It had a putrid smell; and when heated, picoline was evolved, leaving a bulky charcoal. If it is gently heated over an argand lamp, it turns green and iridescent, and dissolves in water with a dull red colour. If bromine is added to an aqueous solution of the hydrate, a resinous precipitate is formed, which dissolves in alcoholic ammonia with a tolerably bright red colour. The red colouring-matter is insoluble in ether.

*Diiodide of picoline methyl iodide*.—This compound was prepared by dissolving iodine in an alcoholic solution of the methyl iodide, and crystallizes out on cooling in beautiful bluish-black feathery plates which transmit red light. It melts at  $129^{\circ}$ . After it had been recrystallized from hot alcohol, it was analyzed by decomposing its solution in alcohol with sulphurous anhydride.

I. 0.2902 grm. of the substance gave 0.4164 grm. of Ag I = 77.54 per cent. of iodine.

II. 0.3066 grm. of the substance gave 0.4459 grm. of Ag I = 78.59 per cent. of iodine;  $C_6H_7N(C_2H_5I)_2$  requires 77.91 per cent. of iodine.

The diiodide of picoline methyl iodide is analogous to the diiodide of the hydriodide. It is soluble in alcohol and ether, very sparingly in carbon disulphide, and insoluble in water. Metallic mercury removes the two additive atoms of iodine, with formation of mercurous iodide.

*Picoline ethyl iodide* has been described by Anderson. There is, however, one reaction which has not before been noticed: if the ethyl hydrate be treated first with bromine and then with ammonia, a brilliant purple colour is produced, which rapidly fades. I may add that I failed to find ethylamine among the decomposition products of the ethyl hydrate.

*Picoline ethene bromide.*—When picoline is mixed with ethene bromide, no reaction takes place till heat is applied. The mixture then deposits a brown oil, which it is impossible to purify. After standing for some months, however, it deposited some crystals, which were separated and repeatedly boiled with animal charcoal, and recrystallized from alcohol. They are small hard prisms, and melt at about  $276^{\circ}$  with decomposition. The chloride crystallizes from alcohol in needles.

*Picoline allyl compounds.*—A mixture of allyl iodide\* and picoline soon becomes turbid and deposits oily drops, which do not solidify at  $0^{\circ}$ . All the picoline allyl compounds (of which the iodide, chloride, nitrate, sulphate, and oxalate were made) are sirupy liquids. The platinochloride is, as usual, crystalline. No diiodide of the hydriodide was obtained; for if equivalent quantities of iodine and picoline allyl iodide are dissolved in alcohol, an oil is formed, from which carbon disulphide does not extract the iodine; nor does the oil dissolve in carbon disulphide. No chlorine addition-product to the allyl chloride could be obtained. The platinum salt appears to be less soluble in water than in alcohol. The platinum was determined with the following results:—

0.2666 grm. of the platinum salt gave 0.0788 grm. of Pt = 29.55 per cent.

Theory for  $\{C_6H_7N(C_3H_5Cl)\}_2 Pt Cl_4$ , 29.12 per cent.

Picoline allyl hydrate is more stable than the ethyl or methyl hydrate. It may be concentrated to a small bulk without becoming discoloured, but when nearly dry it decomposes and turns red. If it is gently heated it becomes iridescent, and then dissolves in alcohol and in ether with a magnificent purple colour, which may be communicated to silk and is apparently fast; it is, however, destroyed by acids, or by reducing agents. It appears to be an oxidation-product of the hydrate.

2. *Compounds of Picoline with Acid Chlorides. Picoline acetyl chloride.*—If acetyl chloride be added to picoline, the whole mass turns brown and chars from the violence of the action; but when dissolved in benzol, a white precipitate is formed, which quickly becomes pink, and finally reddish brown. When both substances are dissolved in chloroform, the solution is green at first, then red, and finally reddish brown. On

\* The allyl iodide was prepared from phosphorus, glycerine, and iodine. One of its properties is, I think, worth mentioning. If it is distilled along with a little water, the mixture boils constantly between  $75^{\circ}$  and  $77^{\circ}$ , whereas each separately boils at about  $100^{\circ}$ . After it was dried over calcium chloride, its boiling-point became normal.

evaporation in a vacuum crystals of a brownish colour were deposited, which deliquesced very rapidly on exposure to air. After they had stood for some weeks over sulphuric acid, the chlorine was estimated.

I. 0.1700 grm. gave 0.1620 of Ag Cl = 22.42 per cent. of chlorine.

II. 0.1070 grm. gave 0.1038 of Ag Cl = 22.13 per cent. of chlorine.

Theory for  $C_6H_7N \cdot C_2H_3OCl$ , 22.83 per cent.

When dissolved in water, picoline hydrochloride and free acetic acid were formed.

The benzoyl chloride was obtained in a similar manner and possessed similar properties. It was not analyzed.

It may perhaps not be uninteresting if I give an account of some attempts made to decompose picoline, all of which, with one exception, led to negative results.

Picoline is not decomposed by potash in any form. Its vapour may be passed over fused caustic potash, or it may be boiled for a month with alcoholic potash, or it may be heated for some days to  $250^\circ$  with alcoholic potash, without undergoing the slightest decomposition. This clearly proves that it has not the nature of a cyanide or of a carbamine.

It is not decomposed by a cherry-red heat, even in presence of lead peroxide or of lime. It may be distilled through a red-hot gun-barrel ten or twelve times without any deposition of carbon, and the picoline which distils over has not even a yellow colour.

It is not attacked, at the temperature of boiling sulphuric acid, by sulphuric acid, nor by a mixture of strong nitric and sulphuric acids.

When picoline nitrate is heated to about  $200^\circ$ , a violent decomposition ensues; red fumes are copiously evolved, and the reaction goes on of its own accord. Unchanged picoline distils over, mixed with oxides of nitrogen and carbonic anhydride in large quantity: the presence of the latter gas was proved by its precipitating an ammoniacal solution of calcium chloride. Thorough decomposition takes place; for nothing but picoline nitrate is left in the retort; and if distilled to dryness, a small carbonaceous residue remains. Anderson states that pyridine nitrate may be sublimed. Picoline is evidently more easily oxidized than pyridine.

Although I am acquainted with Alder Wright's and Dewar's attempts, I believe it is premature to discuss a constitutional formula for picoline. The probabilities appear to me to be

against the hypothesis that it is methyl pyridine. In all methyl compounds with which we are acquainted, chlorine is easily introduced in place of the hydrogen of the methyl group; and, also, the methyl group is usually easily oxidized to a carboxyl group. This is certainly not the case with picoline; for, as Dewar has shown, it yields, when oxidized with potassium permanganate, an acid which he named dicarbopyridenic acid, in which two atoms of hydrogen are replaced by carboxyl groups; and pyridene, in fact, is easily prepared from it by distilling it with lime. Wright supposes that Dewar's picoline may have contained lutidine, and that the acid may have resulted from the oxidation of two hypothetical methyl groups in lutidine. But I have succeeded in preparing dicarbopyridenic acid (a better name for which would be pyridene dicarbonic acid) by a process similar to Dewar's; and I find that from 9 grms. of picoline about 5 grms. of dicarbopyridenic acid are obtained; and surely that is a proof that all the acid is not derived from lutidine. I have already, in the course of some preliminary experiments, made it probable that the alcohol and aldehyde of this acid exist; and I hope to be able to transform the alcoholic into methyl groups, and so produce true dimethyl pyridine. The facts elucidated are these:—(1) The carbon and nitrogen are evidently not combined in the manner in which they exist in a nitrile or in a carbamine; otherwise caustic potash would produce the usual decomposition. (2) Picoline does not contain an amido-group; for not only does it refuse to yield an amido-compound with an acid chloride, but also it is impossible to form a compound with two alcohol radicals. (3) Picoline has the capacity of uniting loosely with two atoms of a halogen, either when it is free, or combined with an acid, or with an alcoholic bromide or iodide. This probably shows that it is an unsaturated compound.

I propose to extend my researches on picoline with the view of ascertaining its real constitution.

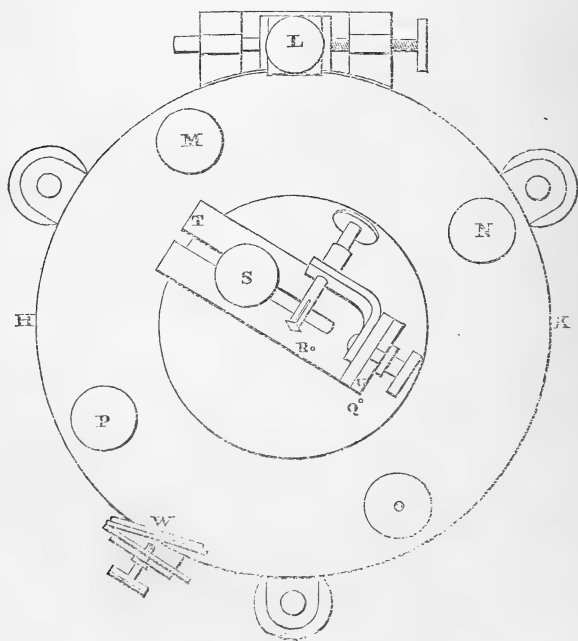
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XXXV. *On a new Form of the Reflective Goniometer.* By W. H. MILLER, M.A., F.R.S., *Fellow of St. John's College, and Professor of Mineralogy in the University of Cambridge* \*.

**A**FTER the publication of a description of Mitscherlich's Goniometer (*Berlin. Abhandl.* 1843, p. 189), and of those employed by Babinet, Haidinger, v. Lang and Schrauf, an account of any other modification of this instrument may appear very superfluous. I believe nevertheless that it is

\* Communicated by the Crystallometric Association.

possible to eliminate some sources of error, and to render the observations more accurate than with the other reflective goniometers in ordinary use, and in some instances to obtain the means of making observations otherwise impracticable. Besides, the construction of the new form is simple, and does not demand any great nicety of workmanship, except of course in the graduation of the circle and verniers. The instrument, made by Messrs. Troughton and Simms, has for its support a tripod with levelling-screws, carrying a fixed disk 200 millims. in diameter and 5 millims. thick, tapped for screws at the points D, E, F, G—by which supports for the vertical mirror, telescope, &c. mentioned later on can be attached to the instrument, in a circle concentric with the disk, having a radius of 88 millims., the diameters through those points making angles of  $45^\circ$  and  $32^\circ$  with a fixed diameter of the disk. The graduated circle, also 200 millims. in diameter and 8 millims. thick, is divided on its edge into spaces of  $20'$  each, the degrees being numbered up to  $360^\circ$ , is read off to  $20''$  by each of two verniers H, K attached to the circular plate, and is provided with a clamp and slow-motion at L. It is tapped for screws at the points M, N, O, P distant  $90^\circ$  from one another, and about 19 millims. from the edge of the circle. A circular disk 100 millims. in diameter, and 8 millims. thick, is tapped for



screws at the points Q, R, S, 38, 13, 25 millims. distant from the centre, for securing the crystal-holder to the instrument in a convenient position. This disk is capable of revolving round a pin the ends of which enter holes in the centres of the disk and graduated circle. A rectangular piece of metal T U, having each arm 95 millims. long, 30 millims. wide and 5 millims. thick, with slits the width of which is very little larger than the diameter of the screw, can be securely clamped to the disk by a screw passing through the slit in the horizontal arm. A bar in which are inserted two screws, by which it can be secured to the vertical branch of T U, carries at its upper end the ordinary Wollaston's branch, to which the crystal is secured by a cement employed by Wollaston, consisting of beeswax melted with a little olive-oil, honey, and lampblack, and stirred while cooling to prevent the separation of the components. The crystal being now adjusted above every part of the instrument, about 195 millims. above the plane on which it rests, is brought into the axis by making T U slide and revolve. The screw being now tightened, the crystal revolves with the circle without any danger of the small angular displacement which is liable to occur, according to Dauber (Poggendorff's *Annalen*, vol. ciii. 1858, p. 107), in the best constructed goniometers when the circle is vertical and the adjustment is by sliders in grooves making right angles with one another. This method of bringing the crystal into the axis of the circle can also be applied with advantage to the small Wollaston's goniometer.

The image of the bright signal is much more distinct when the incidence is as direct as possible, more especially when the face under observation is striated or partially coated with any foreign matter. A small angle of incidence is hardly obtainable with a vertical circle or with a horizontal circle in most localities, as long as the faint signal is seen directly.

Instead of employing an object seen directly for the faint signal, it was suggested independently by A. F. E. Degen (Poggendorff's *Annalen*, vol. xxvii. 1833, p. 557) and E. Sang (Edinb. New Phil. Journ. vol. xxii. 1837, p. 213) that the image of a line seen by reflexion in a mirror could be employed for the faint signal. They failed, however, to obtain the greatest advantage from the employment of the mirror; for though theoretically correct, this is practically inconvenient, because the brightness of the reflection in the mirror very commonly extinguishes the reflected image in the face of the crystal. A better mode of procedure is to place a screen with a narrow vertical slit covered on the side furthest from the observer with thin paper, making at the crystal an angle of from  $3^{\circ}$  to

$5^{\circ}$  with the bright signal, and adopting the image of this in the dark glass mirror W for the faint signal. The mirror, about 40 millims. wide and 30 millims. high, is cemented to a thin metal plate bent so as to make an angle of about  $30^{\circ}$ , while by means of a screw tapped in the other part it can be fastened to the upper end of a strip of brass 208 millims. long, 25 millims. wide and 3 millims. thick, having slits at each end to receive screws, and bent at right angles at a point 40 millims. from the lower end.

By making the mirror revolve through a small angle round the screw which fastens it to its support, and the support round the screw by which it is fastened to the instrument, the mirror is capable of suitable adjustment. A second mirror and support will be found useful, the two mirrors being attached on opposite sides of a vertical plane passing through the centre of the instrument and the source of light. A mean of the results will be free from the errors depending upon the first powers of the eccentricity. For various reasons the accuracy of the observation increases when the angle of incidence of the light on the crystal is small. This angle can be easily reduced to less than  $15^{\circ}$ .

The accuracy of the observations is undoubtedly increased by the use of collimators; they also allow the observation to be made when the space at the observer's disposal is very limited. They may have object-glasses of from 20 millims. to 30 millims. aperture and 500 millims. or more focal length. The two collimators are mounted on supports having each three nearly equidistant notches, so that one resting in the middle notch of each support may have its axis passing through the axis of the goniometer at a distance of 132 millims. above the plane of the graduated circle: it has in its principal focus a vertical adjustable slit, through which the light of the sun is thrown from a heliostat-mirror in the direction of the axis of the collimator. It is sometimes convenient to place a lens in front of the slit in order to enlarge the emergent pencil of light. The other collimator, resting in either pair of the corresponding notches, may have its axis passing nearly through the middle of the dark glass mirror. The end opposite to the object-glass is covered with a plate of glass ground rough on the outer surface, and covered on the inner surface with indian ink scratched away in a vertical diameter.

When the crystal is large, or is implanted on a matrix of some considerable weight, it is obviously impossible to secure and adjust it on the ordinary Wollaston's branch. In such cases it is fixed in a sort of vice having three parallel claws, one of which is movable. By screwing up this claw the



crystal is secured, and then adjusted by making the vice revolve round one or the other of its two axes, making right angles with one another. Also the distance between the faces under observation may be greater than the aperture of the small telescope. The difficulty arising from this circumstance may be overcome by interposing a plate of glass mounted so as to be adjustable in azimuth and zenith-distance, supported by a stem secured to the under plate of the instrument. The observation is made by bringing the image of A seen by reflexion in the face of the crystal into coincidence with the image of the signal B seen by reflexion in the vertical plate of glass.

A small telescope having a power of about 3 may be applied, either for the purpose of observing with greater precision the coincidence of the image of A, as seen by reflexion in a face of the crystal, with the signal B or its image, or of bisecting the reflected image of signal A with the vertical spider line. The telescope is attached to an upright support, the foot of which is fastened to the upper surface of a strip of brass bent twice at right angles in opposite directions, the upper surface of the brass plate being in the plane of the upper surface of the graduated circle, and secured by a screw to the under surface of the base-plate.

By attaching the upright stem of the telescope by one of the screws to the graduated circle, the telescope becomes available for measuring the minimum deviation of a ray of any colour through two inclined faces of a crystal.

The best bright signal is the light of the sun reflected from the mirror of a heliostat through a slit so adjustable that the middle of it remains fixed in space while its diameter admits of being varied. An image of the sun in the focus of a lens of about 30 millims. focal length, formed by the light reflected from a plane mirror, is sufficient in most cases. Much inferior to these is the light of a lamp or that of the sky reflected by a plane mirror through a small opening in a screen. It is hardly necessary to remark that perfect distinctness of vision of the signals, which should be equidistant from the centre of the instrument or very nearly so, is essential to accuracy. When the eye of the observer is not adapted to the distance of the signals, the use of a Galileo's telescope of low power will greatly increase the accuracy of the result.

XXXVI. *Memoir on the three Types of Humite.*

By Professor A. DES CLOISEAUX †.

SCACCHI (Pogg. *Ann. Ergänzungs.* iii. 1851) and vom Rath (Pogg. *Ann. Ergänzungs.* v. 1871) have referred the different forms of Humite to three types belonging to the orthorhombic system, all reducible to the same elements. An examination of the optical characters, however, has led me to separate the three types, and to transfer the crystals belonging to types II. and III. to the clinorhombic system, as shown in the following pages.

Type I. System orthorhombic, with a prism-angle of  $130^{\circ} 19'$ .

$$a : b : c :: 907497 : 420059 : 1849650.$$

Scacchi's symbols.	Symbols of Des Cl.	Miller's symbols.	Calculated angles.	Observed angles. Scacchi.
[ A : B	$pg^1$	001:100	$90^{\circ} 0'$	$90^{\circ} 0'$
A : e	$pe^{\frac{5}{2}}$	001:205	140 49	140 47
A : e <sup>2</sup>	$pe^2$	001:102	134 27	134 30
A : e <sup>3</sup>	$pe^{\frac{3}{2}}$	001:203	126 21	126 17
A : e <sup>4</sup>	$pe^1$	001:101	116 8	116 13
A : e <sup>5</sup>	$pe^{\frac{1}{2}}$	001:201	*103 47	103 47
[ A : i	$pa^5$	001:015	138 38	138 41
A : i <sup>2</sup>	$pa^3$	001:013	*124 16	124 16
A : i <sup>3</sup>	$pa^1$	001:011	102 48	102 50
[ A : r	$pb^{\frac{5}{2}}$	001:115	135 52	135 48
A : r <sup>2</sup>	$pb^2$	001:114	129 30	129 32
A : r <sup>3</sup>	$pb^{\frac{3}{2}}$	001:113	121 44	121 44
A : r <sup>4</sup>	$pb^1$	001:112	112 24	112 23
A : r <sup>5</sup>	$pb^{\frac{1}{2}}$	001:111	101 39	101 41
[ A : o <sub>2</sub>	$pm$	001:100	90 0	90 0
[ B : o	$g^1g^2$	100:310	144 14	144 11
	$g^1g^3$	100:210	132 48	
[ B : o <sup>2</sup>	$g^1m$	100:110	114 50	114 48
[ A : n	$pe_{\frac{1}{3}}$	001:213	116 34	116 30
[ A : n <sup>2</sup>	$pe_3$	001:211	99 28	99 28
	$pg^3$	001:210	90 0	90 0 D.
	$pN$	001:212	108 26	108 20 D.

N(= $b^1b^{\frac{1}{3}}g^{\frac{1}{2}}$ ) and  $g^3$  observed by vom Rath and myself.

$e_3$ (= $b^1b^{\frac{1}{3}}g^1$ ) and  $e_{\frac{1}{3}}$ (= $b^1b^{\frac{1}{3}}g^{\frac{1}{2}}$ ) are the  $x$  and  $\mu$  of figure 227

† Communicated by the Crystallometric Association.

of my 'Manual of Mineralogy.' I have placed the obtuse angle of the prism in front, in accordance with the general usage for orthorhombic prisms; and I have multiplied the old value of the vertical axis by  $\frac{7}{4}$  to simplify the symbols.

The plane of the optic axes is parallel to the base; the acute bisectrix is positive, and is normal to  $h^1(010)$ . Dispersion hardly appreciable in oil,  $\rho < v$  (?).  $2H_{a.r} = 78^\circ 18'$  to  $79^\circ$ .

The laminæ parallel to  $h^1$  consist of plates (of greater or less extent) in which the extinction of the light is complete. These are intersected by patches of irregular form in which the extinction is imperfect, and which probably consist of Humite belonging to the third type. These probably interfere with the accurate determination of the chemical composition of crystals of the first type.

Type II. Yellow Humite from Vesuvius, and brown chondrodite from Sweden (Kafveltorp).

Oblique; prism-angle =  $50^\circ 2' 40''$ .

$b : a : c :: 419122 : 907930 : 696136$ .

Scacchi's symbols.	Symbols of Des Cl.	Miller's symbols.	Calculated angles.	Observed angles. Scacchi.
A : C	$pg^1$	001:010	$90^\circ 0'$	$90^\circ 0'$
	$pe^2$	001:012	141 51	$\begin{cases} 141 & 49 \text{ Scach.} \\ 141 & 48 \text{ Des Cl.} \end{cases}$
$e^2$ pointed out by Scacchi; refound by vom Rath and myself in chondrodite.				
A : i	$pe^1$	001:011	*122 29	122 29
A : e	$pa^1$	001:101	136 0	135 58
A : e'	$po^{\frac{1}{3}}$	001:201	135 56	135 58
	$pa^{\frac{2}{3}}$	001:302	119 56	
	$po^{\frac{1}{6}}$	001:601	119 50	
$o\frac{1}{6}$ and $a^{\frac{2}{3}}$ , vom Rath.				
A : $e^2$	$pa^{\frac{1}{2}}$	001:201	109 5	108 58
A : $e^2$	$ph^1$	001:100	*108 58	108 58
A : m	$pa_2$	001:312	115 1	115 0
	$p\beta$	001:311	95 22	95 20
A : $m^2$	$py$	001:212	125 5	125 5
A : $n^1$	$po_3$	001:211	125 2	125 (nearly) Des Cl.
$o_3$ and $y$ holohedral in Humite (v. Rath) and in chondrodite (Des Cl.).				

Table (continued).

Scacchi's symbols.	Symbols of Des Cl.	Miller's symbols.	Calculated angles.	Observed angles. Scacchi.
A: $n^2$	$p a_3$	$0\bar{0}1:\bar{2}11$	$103^\circ 12'$	$103^\circ 12'$
C: $n^2$	$g^1 a_3$	$010:\bar{2}11$	$135^\circ 40'$	$135^\circ 41'$
C: $n^{2'}$	$g^1 h^3$	$010:210$	$*135^\circ 41'$	$135^\circ 41'$
A: $r$	$p b^1$	$001:\bar{1}12$	$135^\circ 20'$	$135^\circ 18'$
A: $r'$	$p d^{\frac{3}{4}}$	$001:223$	$135^\circ 19'$	$136^\circ$ (nearly) Des Cl.
$b^1$ and $d^{\frac{3}{4}}$ holohedral in Humite (v. Rath) and in chondrodite (Des Cl.).				
A: $r^2$	$p b^{\frac{3}{4}}$	$001:\bar{2}23$	$125^\circ 52'$	
	$p d^{\frac{1}{2}}$	$001:111$	$125^\circ 50'$	$125^\circ 52'$
$b^{\frac{3}{4}}$ and $d^{\frac{1}{2}}$ holohedral in Humite (v. Rath).				
A: $r^3$	$p b^{\frac{1}{2}}$	$001:\bar{1}11$	$113^\circ 28'$	$113^\circ 28'$
	$p b^{\frac{1}{4}}$	$001:221$	$113^\circ 25'$	$113^\circ 10'$ (nearly) D.
$b^{\frac{1}{2}}$ and $d^{\frac{1}{4}}$ holohedral in Humite (v. Rath) and in chondrodite (Des Cl.).				
A: $r^4$	$p m$	$001:110$	$98^\circ 12'$	$98^\circ 18'$

$a_2 = a = (b^1 b^{\frac{1}{2}} h^1)$ ,  $\beta = (b^{\frac{1}{2}} b^{\frac{1}{4}} h^1)$ ,  $\eta = (b^1 b^{\frac{1}{3}} h^{\frac{1}{3}}) = a_3 = (b^1 b^{\frac{1}{3}} h^1) = \rho$ , of which a part  $= h^3$ , are shown in fig. 228 of my 'Manual.'

The plane of the optic axes is inclined from behind forward (from  $a^1$  towards  $o^{\frac{1}{2}}$ ), and makes an angle of about  $30^\circ$  with the base. In chondrodite the twins are of a more or less complex character, and consist of laminæ twinned parallel to the base, which extinguish the light well. The laminæ are associated along undulating surfaces, which cannot be referred to either of the two planes adopted by Scacchi.

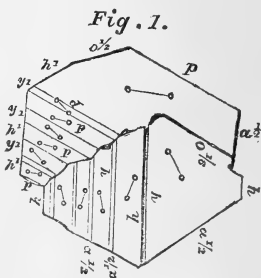
The accompanying figure shows one of the crystals observed by me.

The number and extent of the twin laminæ are different in each specimen; but all obey the same law of association.

The acute bisectrix is positive and normal to the plane of symmetry.

$$2H_{a,r} = 86^\circ 27'.$$

$$2H_{a,b} = 86^\circ 38'; \rho < v, \text{ weak.}$$



Dispersion *tournante* is fairly distinct, when seen in oil, especially in pale yellow crystals. Seven thin laminae of a more or less deep reddish brown showed the dispersion in a manner more or less marked; and the separation of the axes in oil varied for red rays from  $86^{\circ} 14'$  to  $87^{\circ} 20'$ .

Type III. Pale yellow and white Humite from Monte Somma. Oblique, with a prism-angle of  $90^{\circ} 24'$ .

$b : a : c :: 419575 : 907720 : 605135$ .

Seacchi's symbols.	Symbols of Des Cl.	Miller's symbols.	Calculated angles.	Observed angles.
A : c	$pg^1$	001:010	$90^{\circ} 0'$ "	$90^{\circ} 0'$
A : i	$pe^{\frac{3}{2}}$	001:023	136 38	136 35
A : $i^2$	$pe^1$	001:011	*125 13	125 13
A : $i^3$	$pe^{\frac{1}{2}}$	001:021	109 26	109 30
	$pa^{\frac{5}{4}}$	001:405	149 48	
	$po^1$	001:101	149 48	
$o^1$ and $a^{\frac{5}{4}}$ found holohedral (v. Rath).				
A : e	$pa^1$	001:101	143 12 30	143 15
	$po^{\frac{3}{4}}$	001:403	143 11 0	
$a^1$ and $o^{\frac{5}{4}}$ holohedral (v. Rath).				
A : $e^2$	$pa^{\frac{3}{4}}$	001:403	133 40	133 44
A : $e^{2'}$	$po^{\frac{1}{2}}$	001:201	133 40	133 44
A : $e^3$	$pa^{\frac{1}{2}}$	001:201	119 48	119 50
	$po^{\frac{1}{4}}$	001:401	119 48	
$o^{\frac{1}{4}}$ and $a^{\frac{1}{2}}$ holohedral (v. Rath).				
A : $e^4$	$pa^{\frac{1}{4}}$	001:401	100 49	100 48
A : $e^{4'}$	$ph^1$	001:100	*100 48	100 48
A : m	$p\alpha$	001:623	114 55	114 46
A : $m^2$	$p\gamma$	001:621	92 58	92 50
A : n	$p\epsilon$	001:212	132 14	132 7
	$p\epsilon^1$	001:423	132 12	
$\epsilon^1$ and $\epsilon$ holohedral (v. Rath).				
A : $n^2$	$p\lambda$	001:423	122 57	123 0
	$po_3$	001:211	122 56	
$\lambda$ and $o_3$ holohedral (v. Rath).				
A : $n^3$	$pa_3$	001:211	111 15	111 18
A : $n^{3'}$	$p\pi$	001:421	111 14	111 18
	$p^3\zeta$	001:141	105 10	
$\zeta$ , new form (v. Rath).				

Table (continued).

Seacchi's symbols.	Symbols of Des Cl.	Miller's symbols.	Calculated angles.	Observed angles.
$A : n^4$	$p \omega$	$001 : \bar{4}21$	$97^\circ 23'$	$97^\circ 25'$
$A : n^{4'}$	$p k^3$	$001 : 210$	$97^\circ 23'$	$97^\circ 25'$
$A : r$	$p b^1$	$001 : \bar{1}12$	$140^\circ 14'$	$140^\circ 20'$
$A : r^2$	$p d^{\frac{3}{4}}$	$001 : 223$	$136^\circ 9'$	$136^\circ 8'$
$A : r^3$	$p b^{\frac{3}{4}}$	$001 : \bar{2}23$	$131^\circ 23'$	$131^\circ 25'$
$A : r^4$	$p d^{\frac{1}{2}}$	$001 : 111$	$125^\circ 47'$	$125^\circ 50'$
$C : r^4$	$g^1 d^{\frac{1}{2}}$	$010 : 111$	$137^\circ 25'$	$137^\circ 28'$
$A : r^5$	$p b^{\frac{1}{2}}$	$001 : \bar{1}11$	$119^\circ 17'$	$119^\circ 20'$
$A : r^6$	$p d^{\frac{1}{4}}$	$001 : 221$	$111^\circ 49'$	$111^\circ 53'$
$A : r^7$	$p b^{\frac{1}{4}}$	$001 : \bar{2}21$	$103^\circ 31'$	$103^\circ 37'$
$A : r^8$	$p m$	$001 : 110$	$94^\circ 35'$	$94^\circ 28'$
$C : r^8$	$g^1 m$	$010 : 110$	$*154^\circ 48'$	$154^\circ 48'$

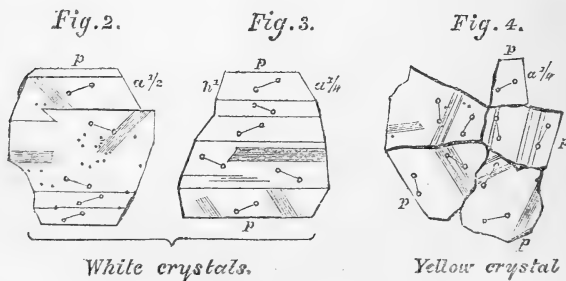
$\alpha$ ,  $\gamma$ ,  $\epsilon$ ,  $\lambda$ ,  $a_3 = \tau_1$  posterior,  $\tau'_1 = \tau_1$  anterior, are represented in fig. 229 of my 'Manual;'  $\zeta$ ,  $\epsilon^1$ ,  $o_3$ , and  $\omega$  are new forms found by vom Rath.

Plane of the optic axes inclined *from behind forward*, making an angle with the base of about  $11^\circ$ . The acute bisectrix is positive and normal to the plane of symmetry. Dispersion of the axes very weak,  $\rho < v$ . Dispersion *tournante* hardly appreciable in the most homogeneous plates.

$2H_{a,r} = 84^\circ 38'$  to  $85^\circ 4'$  in white crystals,

$86^\circ 40'$  to  $87^\circ 14'$  in a yellow crystal from Monte Somma.

Internal structure more or less complex, formed by the union parallel to the base of twin laminae (figs. 2 and 3). These



bands extinguish the light distinctly, but are penetrated by narrow bandelets which do not extinguish the light, and which make with the base an angle of about  $60^\circ$ , and appear to be parallel to the faces  $e^3 = a^{\frac{1}{2}}$ . One yellow crystal (fig. 4), very distinctly twinned, as shown by its very decided reentrant angles, consists of five individuals united in the interior along perfectly irregular surfaces. Each of the members of this twin, except the upper small one, contains both bands parallel to  $p$  and bandelets parallel to  $a^{\frac{1}{2}}$  of the white crystals.

It seems to me that we might retain the name *Humite* for the orthorhombic crystals of type I., that of *chondrodite* for the clinorhombic crystals of type II., and seek a name for the crystals of type III.—*clinohumite*, until a better be found. It ought, however, to be ascertained if all the crystals from Sweden and from America belong to type II., or if the brown crystals from Kafveltorp alone belong to this type, while the grey or brownish ones from Ladugrufvan and Pargas are of type III. (Edward Dana admits the last two types in American crystals). It is, however, evident that there is a close crystallographic and chemical relationship between the second and third types, and that they differ most in their optical properties, although Websky tries to show a chemical difference by means of new formulæ, which I declare myself incapable of following. This point will be understood in time; but what was important was to establish first the undoubted facts, and the non-existence of three types of one and the same species, which had always seemed to me an extraordinary thing, difficult to admit, especially in presence of the holohedrism of the one, and the hemihedrism of the two others.

XXXVII. *Note on Spherical Harmonics*. By J. J. SYLVESTER, F.R.S., Professor of Mathematics at the Johns Hopkins University, Baltimore\*.

IF for a moment we confine our attention to so-called "zonal" harmonics, and affect each element of a uniform spherical shell with a density varying as the product of two such harmonics of unequal degrees, we know that the mass of such shell is zero. A very slight consideration will serve to show that this is tantamount to affirming that if a given spherical surface be charged with a density inversely proportional to the product of the distances of each element from two fixed internal points lying in the same radius produced, then the mass of such shell will be a complete function of the

\* Communicated by the Author.

product of the distances of the two points from the centre; and in fact, if we write  $dS$  for an element of a spherical surface, it is easy to find, by direct integration, that

$$\iint \frac{dS}{\sqrt{c^2 - 2hx + h^2} \sqrt{c^2 - 2h'x + h'^2}}$$

for the entire surface is proportional to

$$\frac{1}{\sqrt{hh'}} \log \frac{c^2 - \sqrt{hh'}}{c^2 + \sqrt{hh'}}.$$

In like manner, the truth of the more general theorem relating to the surface-integral of the product of any two harmonics of unequal degrees involves, and is involved in, the fact that the surface  $\iint \frac{dS}{R \cdot R'}$ , where

$$R^2 = (x-h)^2 + (y-k)^2 + (z-l)^2,$$

$$R'^2 = (x-h')^2 + (y-k')^2 + (z-l')^2$$

and  $h^2 + k^2 + l^2$  and  $h'^2 + k'^2 + l'^2$  are each less or each greater than the square of the radius of the sphere, is not merely a function (as we see *a priori* from the symmetry of the sphere must be the case) of the three quantities

$$h^2 + k^2 + l^2, \quad h'^2 + k'^2 + l'^2, \quad hh' + kk' + ll',$$

but, more definitely, is a complete function of the product of two of them, viz.  $(h^2 + k^2 + l^2)(h'^2 + k'^2 + l'^2)$ , and of the third. In other words, the fundamental law of spherical harmonics is exactly tantamount to the assertion that if each element of a sphere is charged with a density proportional to the product of its distances from two internal or two external points, then the mass of the sphere will be a function only of the density at the centre and of the angle subtended at the centre by the line joining the given pair of points; or, venturing upon an irrepressible neologism, which explains its own meaning, the *Bipotential*, with respect to a given uniform sphere at any point-pair, is a function only of the Bipotential thereat with respect to a unit particle at the centre, and of the angle subtended at the centre by the line joining the two given points. Of course, if this is true for the volume of the sphere, it must be true for any shell of uniform thickness, or, in other words, for the surface, and *vice versa*. In what immediately follows the volume of a spherical shell is to be understood. It is, I think, very noticeable that in that proof no process whatever of integration is employed; only the *idea* implied in integration is employed to acquire the fact that the integral in question cannot but be a function of three parts of the triangle, of which the centre of



the sphere and the two given points are the apices. The rest of the proof follows as a matter of purely formal or algebraical necessity from the above fact, conjoined with that of each factor under the sign of integration being subject to Laplace's equation. In this feature of exemption from all use of integration as a process, this proof, I believe, stands alone.

It is further remarkable that its success depends on the proposition being stated as a whole; it would not be applicable, for example, to the simple case, taken *per se*, treated of at the beginning of this paper. It is by no means uncommon in mathematical investigation for this to happen, and (as regards the exigencies of reasoning) for the part to be in a sense greater than the whole—the groundwork of this wonder-striking intellectual phenomenon being that, for mathematical purposes, all quantities and relations ought to be considered (so experience teaches) as in a state of flux. In the particular case before us it is not difficult to see *à priori* why the general proposition should be more easily demonstrable than any special case of it, the reason being that more information as to the *form* of the function under consideration is made use of in dealing with the general than in dealing with any special case.

The integral under consideration is

$$\iiint \frac{dV}{RR'} \text{ (say I),}$$

where

$$R^2 = x^2 + y^2 + z^2 - 2hx - 2ky - 2lz + h^2 + k^2 + l^2,$$

$$R'^2 = x'^2 + y'^2 + z'^2 - 2h'x - 2k'y - 2l'z + h'^2 + k'^2 + l'^2.$$

Call

$$h^2 + k^2 + l^2 = r^2, \quad h'^2 + k'^2 + l'^2 = t^2, \quad hh' + kk' + ll' = s.$$

Then  $\frac{1}{RR'}$ , expanded under the form of a converging series ( $x, y, z$  being for a moment regarded as constants), will be of the form  $\frac{1}{rt}$  multiplied by a rational function of  $\frac{1}{r}, \frac{h}{r^2}, \frac{k}{r^2}, \frac{l}{r^2}$  and of  $\frac{1}{t}, \frac{h'}{t^2}, \frac{k'}{t^2}, \frac{l'}{t^2}$  when the two points are external, and (more simply) of  $h, k, l$  and of  $h', k', l'$  when they are both internal. I, we know, must turn out to be a complete function of  $r, s, t$ , and, when expressed in the form of a series derived from the above expansion, will be the sum of terms of the form  $r^i \cdot s^j \cdot t^k$ , where it is obvious that  $i$  and  $j$  must both be negative when the "pair-point" is exterior, both positive when it is interior to the shell, and one positive and one negative in the remaining case.

Now we have identically

$$\left(h \frac{d}{dk} - k \frac{d}{dh}\right)r=0,$$

$$\left(h' \frac{d}{dk'} - k' \frac{d}{dh'}\right)t=0,$$

and

$$\left(\left(h \frac{d}{dk} - k \frac{d}{dh}\right) + \left(h' \frac{d}{dk'} - k' \frac{d}{dh'}\right)\right)s=0.$$

Hence with respect to I as operand we have

$$\left(h \frac{d}{dk} - k \frac{d}{dh}\right) + \left(h' \frac{d}{dk'} - k' \frac{d}{dh'}\right)=0.$$

Operate on this identity with

$$\left(h \frac{d}{dk} - k \frac{d}{dh}\right) - \left(h' \frac{d}{dk'} - k' \frac{d}{dh'}\right),$$

and we obtain

$$\begin{aligned} &\left(h \frac{d}{dk} - k \frac{d}{dh}\right)^2 - \left(h \frac{d}{dh} + k \frac{d}{dk}\right) \\ &= \left(h' \frac{d}{dk'} - k' \frac{d}{dh'}\right)^2 - \left(h' \frac{d}{dh'} + k' \frac{d}{dk'}\right); \end{aligned}$$

and there will be two other equations of like form. Adding all these together, changing all the signs, and remembering that in regard to I as operand

$$\left(\frac{d}{dh}\right)^2 + \left(\frac{d}{dk}\right)^2 = -\left(\frac{d}{dl}\right)^2,$$

$$\left(\frac{d}{dh'}\right)^2 + \left(\frac{d}{dk'}\right)^2 = -\left(\frac{d}{dl'}\right)^2,$$

we obtain

$$\begin{aligned} &\left(h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl}\right)^2 + 2\left(h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl}\right) \\ &= \left(h' \frac{d}{dh'} + k' \frac{d}{dk'} + l' \frac{d}{dl'}\right)^2 + 2\left(h' \frac{d}{dh'} + k' \frac{d}{dk'} + l' \frac{d}{dl'}\right). \end{aligned}$$

In this formula

$$\left(h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl}\right)^2$$

stands for its algebraical value

$$h^2\left(\frac{d}{dh}\right)^2 + 2hk \frac{d}{dh} \frac{d}{dk} + \dots;$$

but if we write

$$\left( \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right)^* \right)^2$$

to denote the operation twice repeated, then

$$\begin{aligned} & \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right)^2 \\ &= \left( \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right)^* \right)^2 - \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right), \end{aligned}$$

and so for the like expressions with the accented letters. The formula thus is

$$\begin{aligned} & \left( \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right)^* \right)^2 + \left( h \frac{d}{dh} + k \frac{d}{dk} + l \frac{d}{dl} \right) \\ &= \left( \left( h' \frac{d}{dh'} + k' \frac{d}{dk'} + l' \frac{d}{dl'} \right)^* \right)^2 + \left( h' \frac{d}{dh'} + k' \frac{d}{dk'} + l' \frac{d}{dl'} \right); \end{aligned}$$

or say

$$((E*)^2 + E - (E'*)^2 - E')I = 0,$$

or simply

$$(F - F')I = 0.$$

Let now  $r^i s^j t^k$  be any term in  $I$ ; then since

$$\begin{aligned} E r &= r, & E s &= s, & E t &= 0, \\ E' t &= t, & E' s &= s, & E' r &= 0, \end{aligned}$$

we have

$$\begin{aligned} F r^i s^j t^k &= ((i+j)^2 + (i+j)) r^i s^j t^k, \\ F' r^i s^j t^k &= ((k+j)^2 + (k+j)) r^i s^j t^k, \end{aligned}$$

and thence

$$(F - F') r^i s^j t^k = (i^2 + i + 2ij - k^2 - k - 2kj) r^i s^j t^k.$$

Hence  $\Sigma(i^2 + i + 2ij - k^2 - k - 2kj) r^i s^j t^k$  must be identically zero; therefore  $i - k = 0$ , or  $i + k + 2j + 1 = 0$ .

But when the two points to which the Bipotential is referred (and which I shall hereafter call the points of *prise*) are both external or both internal,  $i$  and  $k$  have the same sign; therefore  $i = k$ , and the integral is a function only of  $rs$  and  $t$ , or say of

$$(h^2 + k^2 + l^2)(h'^2 + k'^2 + l'^2), \quad (hh' + kk' + ll')^*.$$

\* When the point corresponding to  $r$  is external and that corresponding to  $t$  is internal, the equation  $i + k + 2j + 1 = 0$  applies, which shows that each term is of the form  $\frac{1}{r} \left( \frac{t}{r} \right)^\lambda \left( \frac{s}{rt} \right)^\mu$ ; that is to say, the Bipotential mul-

tiplied by  $r$  is a complete function of  $\frac{t}{r}$  and the cosine of the angle which the line joining the two fixed points subtends at the centre.

Thus the desired theorem has been established by virtue of an algebraical necessity of form alone; and the proof is of course applicable to space in any number of dimensions, substituting for the sphere or spherical surface its analogue in such space, and for the reciprocal of distance the proper power necessary for the satisfaction of Laplace's equation, *i. e.* the  $(q-2)$ th power of the reciprocal, where  $q$  is the number of dimensions (supposed to be greater than 2).

For the case of two dimensions, substituting the logarithm for the reciprocal, so that *ex. gr.* we are able to affirm that if each element of a circular ring be affected with a density proportional to the product of the logarithms of its distances from two fixed internal points, the mass of such ring will depend only on the product of their distances from the centre of the ring and the angle between these distances—for this case, writing  $E = h \frac{d}{dk} + k \frac{d}{dk}$  and  $E' = h' \frac{d}{dk'} + k' \frac{d}{dk'}$  in the equation  $(F - F')I = 0$ ,  $F = (E*)^2$  and  $F' = (E'*)^2$ ; and if the two points are interior, every term in  $\frac{1}{RR'}$  will be of the

form  $C r^i \cdot s^j \cdot t^k$ ,  $i$  and  $k$  being both positive, and we must have  $i^2 + 2ij - k^2 - 2kj = 0$ , and consequently  $i = k$ —the other solution,  $i + k + 2j = 0$ , being applicable to the case of one point being external and the other internal. If the points are both external there will be four sets of terms. One set will consist of the single term  $A \log r \log t$ ; a second, of terms of the form  $c \log r \cdot r^i s^j t^k$ ; a third, of terms of the form  $c \log t \cdot r^i s^j t^k$ ; and the last set, of terms of the form  $c r^i s^j t^k$ : and it is easy to see that  $F(\log r \log t) = 0$ ,  $F'(\log r \log t) = 0$ ,

$$(F - F') \log r \cdot r^i s^j t^k$$

$$= ((i+j)^2 \log r - (k+j)^2 \log r + 2(i-k)) r^i s^j t^k,$$

and consequently  $i = k$  for the second and third set; as regards the fourth set,  $i = k$  for the same reason as in the case of three dimensions. Hence

$$I = A \log r \log t + \log r \phi(rt, s) + \log t \psi(rt, s) + \omega(rt, s);$$

and as  $r$  and  $t$  are interchangeable, we must have  $\phi = \psi$ , and consequently

$$I - A \log r \log t = F(rt, s);$$

so that not now the mass of the ring, but the difference between it and the mass due to the density at the centre is invariable when  $rt$  and  $s$  are given.

For greater simplicity, and as bearing more immediately on the theory of spherical harmonics, I have hitherto regarded the points of the pair-point at which the “bipotential” is

reckoned either both internal or both external. The results established in these two cases are not complementary, but mutually equivalent to each other, and to the theorem that the integral along a spherical surface of the product of two spherical harmonics of unequal degrees is zero. In the third case, where one point is internal and the other external, then for the case of space of three dimensions the equation between  $i$  and  $k$  will have to be satisfied, not by  $i=k$  but by  $i+k+2j+1=0$ , as previously stated in a footnote; and for two dimensions the equation would have to be satisfied, not by  $i=k$  but by  $i+k+2j=0$ .

The advantage of the method here indicated is that it is immediately applicable to space of any number of dimensions. I shall now proceed to show that it leads at once to the determination of the values of the surface-integral of the product of any two given types of spherical harmonics of equal degrees, and *mutatis mutandis* to the corresponding surface-integral in space of any order.

To prove that the degrees must be equal or else the integral will vanish, we have combined the two Laplacian operators applicable to  $R$  and  $R'$  respectively; to find the value of the integral in a series, I use either of these operators to act singly on the result acquired by their use in combination. For greater simplicity suppose the point-pair to be internal; then, calling

$$a+b+c=\mu=\alpha+\beta+\gamma,$$

the problem to be solved is in effect that of finding the value of the numerical coefficient of  $h^a k^b l^c . h'^a k'^b l'^c$  in the integral  $I$ . Now we know by what precedes that the value, say  $I_\mu$ , of that part of  $I$  which is of the  $\mu$ th order in the two sets  $h, k, l$ ;  $h', k', l'$  respectively is a rational function of  $rt$  and  $s$ ; and we may accordingly write

$$I_\mu = A s^\mu + B s^{\mu-2} . \theta + C s^{\mu-4} . \theta^2 + \dots,$$

where

$$s = hh' + kk' + ll',$$

and

$$\theta = (h^2 + k^2 + l^2)(h'^2 + k'^2 + l'^2) = \rho\rho'.$$

When  $A, B, C, \dots$  are determined, the problem is virtually solved, as we shall then know the coefficient of

$$h^a k^b l^c . h'^a k'^b l'^c$$

by mere binomial expansions.

Since

$$\left(\frac{d}{dh}\right)^2 + \left(\frac{d}{dk}\right)^2 + \left(\frac{d}{dl}\right)^2,$$



I becomes the surface-integral of  $\frac{1}{1-2hx+h^2}$ , and is equal to

$$\begin{aligned} 2\pi \int_{-1}^1 \frac{dx}{1-2hx+h^2} &= \frac{2\pi}{h} \log \left( \frac{1+h}{1-h} \right) \\ &= 4\pi \left( 1 + \frac{h^2}{3} + \dots \frac{h^{2\mu}}{2\mu+1} + \dots \right). \end{aligned}$$

Therefore

$$A+B+C+\dots = \frac{4\pi}{2\mu+1},$$

or

$$S_\mu A = \frac{4\pi}{2\mu+1},$$

where

$$\begin{aligned} S_\mu &= 1 - \frac{\mu(\mu-1)}{2(2\mu-1)} + \frac{\mu(\mu-1)(\mu-2)(\mu-3)}{2 \cdot 4 \cdot (2\mu-1)(2\mu-3)} \\ &\quad - \frac{\mu(\mu-1)(\mu-2)(\mu-3)(\mu-4)(\mu-5)}{2 \cdot 4 \cdot 6 \cdot (2\mu-1)(2\mu-3)(2\mu-5)} \dots \end{aligned}$$

This series admits of summation. And I find

$$\begin{aligned} S_1 &= 1, \quad S_2 = \frac{2}{3}, \quad S_3 = \frac{2}{5}, \quad S_4 = \frac{8}{35}, \quad S_5 = \frac{8}{63}, \quad S_6 = \frac{16}{3 \cdot 7 \cdot 11}, \\ S_7 &= \frac{16}{3 \cdot 11 \cdot 13}, \quad S_8 = \frac{128}{3 \cdot 11 \cdot 13 \cdot 15}, \quad S_9 = \frac{120}{5 \cdot 11 \cdot 13 \cdot 17}, \\ S_{10} &= \frac{256}{11 \cdot 13 \cdot 17 \cdot 19} \dots \end{aligned}$$

and in general

$$S_{2m} = \frac{2 \cdot 4 \cdot 6 \dots (2m)}{(2m+1)(2m+3)(2m+5) \dots (4m-1)}$$

and

$$S_{2m+1} = \frac{2m+1}{4m+1} S_{2m};$$

that is to say,  $S_\mu$  is the reciprocal of the coefficient of  $h^\mu$  in  $(1-2h)^{-\frac{1}{2}}$ .

Hence the values of A, B, C... in  $I_\mu$  are completely determined, and  $I_\mu$ , and consequently the value of the complete integral of

$$\iint dS \left( \left( \frac{d}{dx} \right)^a \left( \frac{d}{dy} \right)^b \left( \frac{d}{dz} \right)^c \cdot \frac{1}{r} \right) \left( \left( \frac{d}{dx} \right)^a \left( \frac{d}{dy} \right)^\beta \left( \frac{d}{dz} \right)^\gamma \cdot \frac{1}{r} \right),$$

is known for all values of  $a, b, c$ ;  $\alpha, \beta, \gamma$ —and this by a method which is applicable step by step to any number of vari-

ables, provided in place of  $\frac{1}{r}$  we write  $\frac{1}{r^{n-2}}$  when  $n$  exceeds 2, and  $\log r$  when  $n=2$ , and consider  $dS$  to be the element of what in  $n$  dimensions corresponds to a spherical surface in three-dimensional space.

The method employed, of first using two Laplacian operators in combination to determine one property of the form under investigation and then a single one of them to act on the form thus partially determined, reminds one very much of the method for obtaining invariants of given orders from their two general partial differential equations. Combined, these two equations express the law of isobarism; then, assuming the isobarism, a single one of the two serves to determine the special values of the coefficients. The analogy between that process and the one here employed seems to me to be exact, although the subject-matter is so very unlike in the two problems—and is the more interesting on that very account.

The bipotential in the case where the two points of *prise* are both internal being known under the form  $F\left(\frac{rr'}{a^2}, \cos \alpha\right)$ , where  $a$  is the radius of the sphere, its value for the case where these points are both external, and for the case where they are one internal and the other external, may be assigned without any further calculation as follows:—

1. Suppose  $r$  greater than the radius of the sphere, but  $r'$  less. We know *à priori* from the result previously obtained (and stated in a footnote), that the bipotential for this case is of the form  $\frac{1}{r} G\left(\frac{r'}{r}, \cos \alpha\right)$ . Now in place of  $r$ ,  $r'$  substitute  $a$ ,  $\frac{ar'}{r}$ ; then the bipotential becomes  $\frac{1}{a} G\left(\frac{r'}{r}, \cos \alpha\right)$ .

But we may by an easily justifiable application of the principle of continuity now regard  $a$  (as well as  $\frac{ar'}{r}$ ) as the distance of an internal point from the centre. Hence we have

$$\frac{1}{a} G\left(\frac{r'}{r}, \cos \alpha\right) = F\left(\frac{r'}{r}, \cos \alpha\right),$$

or

$$\frac{1}{r} G\left(\frac{r'}{r}, \cos \alpha\right) = \frac{a}{r} F\left(\frac{r'}{r}, \cos \alpha\right),$$

which is the value of the bipotential of a spherical surface cut by the line of *prise*,  $r$  being the distance of the external point of *prise* from the centre.

2. Suppose  $r$  and  $r'$  to be each greater than the radius, and



$r > r'$ ; we know the bipotential is of the form  $H\left(\frac{rr'}{a^2}, \cos \alpha\right)$ .

For  $r, r'$  substitute respectively  $\frac{rr'}{a}$ . Then we may regard the case as that of an exterior and interior point of *prise*, and consequently from the last case we have

$$H\left(\frac{rr'}{a^2}, \cos \alpha\right) = \frac{a^2}{rr'} F\left(\frac{a^2}{rr'}, \cos \alpha\right).$$

If we compare the two expressions  $F\left(\frac{rr'}{a^2}, \cos \alpha\right)$  and

$$\frac{a^2}{rr'} F\left(\frac{a^2}{rr'}, \cos \alpha\right)$$

respectively applicable to two internal and two external points of *prise*, it will easily be seen that it leads to the following theorem. Let there be two concentric spheres, and let any two radii cut the first and second surfaces in the points P, Q and P', Q' respectively; then the bipotential of the first surface with respect to P', Q' as the points of *prise*, is to the bipotential of the second surface with respect to P, Q as the points of *prise* in the ratio of the squares of the radii of the two surfaces to each other.

This is a theorem of precisely the same kind as Ivory's for the comparison of the attractions (or, if we please, the potentials) of two confocal ellipsoids in the particular case when they become two concentric spheres, and may be verified by precisely the same geometrical method. For we have only to divide the two spherical surfaces into corresponding elements  $m, m'$  by radii drawn in all directions to meet the two surfaces, and it is evident that we shall have the distances  $mP'$  and  $m'P$  equal, as also  $mQ'$  and  $m'Q$ . And, moreover, the ratio of any two corresponding elements  $m, m'$  will be as the square of the radii, which evidently establishes the theorem in question. It may further be noticed that the relation between the bipotentials in the three several cases considered may be deduced from the fact that each such radical as

$$\frac{1}{\sqrt{1-2hx-2ky-2lz+h^2+k^2+l^2}},$$

where  $h^2+k^2+l^2$  is greater than unity, may be put under the form

$$\frac{1}{\sqrt{h^2+k^2+l^2}} \frac{1}{\sqrt{1-2h_1x-2k_1y-2l_1z+h_1^2+k_1^2+l_1^2}},$$

where  $h_1, k_1, l_1$  and  $h, k, l$  are the coordinates of two points the

inverses (or electrical images) of each other in regard to the origin, and consequently  $h_j^2 + k_j^2 + l_j^2$  less than unity. This is going to the heart of the matter. So I may observe that if we would go to the root of the relation between positive- and negative-degreed solid spherical harmonics, the more logical mode of proceeding is not (as is usually done) to infer this by a lengthy *à posteriori* process, but immediately from the fact that since

$$\frac{1}{\sqrt{(x^2 + y^2 + z^2) - 2(hx + ky + lz) + (h^2 + k^2 + l^2)}}$$

is nullified by the operator

$$\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2,$$

so also must the same operator nullify the radical

$$\frac{1}{\sqrt{1 - 2(hx + ky + lz) + (h^2 + k^2 + l^2)(x^2 + y^2 + z^2)}}.$$

Before proceeding further, I ought to observe that H in the above series for the bipotential may easily be shown to be

$$\frac{4\pi}{2\mu + 1} \text{ multiplied into the coefficient of } t^\mu \text{ in the expansion of } \frac{1}{\sqrt{1 - 2st + \theta t^2}}^* ; \text{ or, in other words, if the distances from the}$$

\* *s*, it will be remembered, is  $hh' + kk' + ll'$ , and  $\theta$  is the product  $(h^2 + k^2 + l^2)(h'^2 + k'^2 + l'^2)$ . The statement in the text follows as a consequence from the fact that  $(1 - 2st + \theta t^2)^{-\frac{1}{2}}$  obeys Laplace's law, and, when expanded according to powers of  $t$ , is of the form found for  $I_\mu$ , and must consequently be identical with it to a factor *près*, that factor being a function of  $\mu$ , whose value is easily found by making  $h = h'$  and  $k, l; k', l'$  each zero. In like manner it may be shown that in higher space of  $n$  dimensions the corresponding value of  $H_\mu$  is a function of  $\mu$  multiplied by the coefficient of  $t^\mu$  in  $(1 - 2\Sigma hh't + (\Sigma h^2)\Sigma(h'^2)t^2)^{1-\frac{1}{2}n}$ ; and I find that this function, say  $\phi(\mu, n)$ , (as will be shown in a sequel to this paper) is always a rational function in  $m$ , containing in the denominator, when  $n$  is odd, one factor of the form  $2m + j$ , all the others being of the form  $m + i$ —and when  $n$  is even, factors all of the form  $m + i$ . Whatever the form of these linear factors had been for even numbers, we could see *à priori* that the Bipotential for space of even dimensions could contain only algebraic and inverse circular or logarithmic functions. But as regards the case of space of odd dimensions, the fact of there being no factors except of the form  $m + i, 2m + j$ , is prepotent in determining the form of the result. For space of two dimensions the Bipotential does not appear readily to yield to summation in finite terms. Thus at one blow the theory of spherical harmonics has been extended to “globoidal” harmonics in general; and the chief cases of statical distribution of electricity heretofore solved may be regarded as virtually solved *mutatis mutandis* for space of any number of dimensions, of course with the proviso that the law of attraction (in consonance with

centre of a spherical surface of two points *in the interior* be  $r, r'$ , and the angle which the line joining them subtends at the centre be  $\omega$ , then the value of the bipotential of the surface at this point-pair is the elliptic integral

$$\int_0^{\frac{\sqrt{rt}}{c}} \frac{4\pi dx}{\sqrt{1-2x^2\cos\omega+x^4}},$$

which I take leave to call the Cardinal Theorem of Spherical Harmonics; for it is the theorem from which spring all the properties relating to the "surface-integral" of the product of any two rational forms of Laplace's coefficients.

the hypothetical principle of force-emanation to which the English school of physicists seem to be returning) is always to be supposed to vary as the  $(i-1)$ th power of the distance in space of  $i$  dimensions.

The actual expression for  $\phi(m, n)$  when  $n$  is 3 we know is  $\frac{4\pi}{2m+1}$ . In general when  $n$  is any other odd number, I find that its value is

$$\frac{2(2\pi)^{\frac{n-1}{2}}}{(2m+n-2)(m+n-3)(m+n-4)\dots\left(m+\frac{n-1}{2}\right)}.$$

As this expression may be split up into partial fractions, it is obvious that the value of the Bipotential may be expressed by means of the sum of integrals of the form

$$\int_{-\infty}^1 \frac{u \, du}{(\sqrt{u^2+Au+B})^{n-2}},$$

and one of the form

$$\int_{-\infty}^1 \frac{du}{(\sqrt{u^2+Au+B})^{n-2}};$$

so that it involves no transcendents of a higher order than an ordinary elliptic function. I think also that it follows from the limits to the value of  $j$  that the other integrals are mere algebraical functions. The less interesting case when  $n$  is an even number (being very much pressed for time and within twenty-four hours of steaming back to Baltimore) I have not taken the trouble to work out in detail.

The determination of the Bipotential constitutes in itself a vast accession to the theory of definite integrals, and promises to be fruitful in yielding whole new families of such when subjected to the usual processes performed under the sign of integration. But does the theory stop here? The success of my method for the Bipotential depends solely upon the discovery that, as regards internal points of *prise*, it may be regarded as a function of only two variables,  $rr'$  and  $\cos \omega$ . Now a Tripotential will obviously at first sight be a function of not more than six variables, viz. the three quantities  $r, r', r''$  and the cosines of the angles between them; but it becomes a question whether this number also may not be reduced to be less than six, themselves simple functions of the six parts of a tetrahedron; and so for a multipotential of any order the question arises, Is it a function of  $\frac{1}{2}m(m+1)$  quantities or of a smaller number? and if so, of what number of what variables?

Since every spherical harmonic of integral degree is a linear function of the differential derivatives of  $(x^2 + y^2 + z^2)^{-\frac{1}{2}}$ , the whole theory of the diplo-spherical-harmonic-surface integral is contained in the annexed equation, which springs immediately from the expression found above for the bipotential of a spherical surface at two internal points (slightly modified by taking  $-h, -k, -l; -h', -k', -l'$  for the coordinates of the points) by means of the simple and familiar principle that any differential derivative with respect to  $x, y, z$  of a function of  $x, y, z$  is identical with what the corresponding derivative with respect to  $h, k, l$  of the like function of  $x + h, y + k, z + l$  becomes when  $h, k, l$  are made to vanish.

Let  $U$  stand for  $u^4 - 2\Sigma hh'. u^2 + \Sigma h^2. \Sigma h'^2$ , and let

$$V(h, k, l; h', k', l') = \int_{\infty}^1 \Phi\left(\frac{d}{dh}, \frac{d}{dk}, \frac{d}{dl}\right) \Psi\left(\frac{d}{dh'}, \frac{d}{dk'}, \frac{d}{dl'}\right) \frac{du}{\sqrt{U}},$$

where  $\Phi$  and  $\Psi$  are forms of function which denote series, whether finite or infinite, containing only positive integer powers of the variables. Then, if  $\rho = \frac{1}{\sqrt{x^2 + y^2 + z^2}}$  and  $dS$  is the element of a spherical surface of unit radius, the complete integral

$$\begin{aligned} \iint dS \left( \Phi\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}\right) \rho \Psi\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}\right) \rho \right) \\ = 4\pi V(0, 0, 0; 0, 0, 0). \end{aligned}$$

When  $\Phi$  and  $\Psi$  are homogeneous forms of function, each of the degree  $i$ , if we write

$$T = 1 - 2\Sigma hh' + \Sigma h^2. \Sigma h'^2,$$

and make

$$\Omega(h, k, l; h', k', l') = \Phi\left(\frac{d}{dh}, \frac{d}{dk}, \frac{d}{dl}\right) \Psi\left(\frac{d}{dh'}, \frac{d}{dk'}, \frac{d}{dl'}\right) \frac{1}{\sqrt{T}},$$

the value of the corresponding harmonic-surface integral becomes

$$\frac{4\pi}{2m+1} \Omega(0, 0, 0; 0, 0, 0).$$

I am not aware that a rule for finding such integral so simple in form and of such absolute generality in operation as the one above has been given before; the interesting rule furnished by Professor Clerk-Maxwell, 'Electricity and Magnetism' (vol. i. p. 170), assumes that  $\Phi$  and  $\Psi$  have been each reduced to the form of the product of linear functions of  $\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$ —a reduction which cannot practically be effected,

as it involves the solution of systems of equations of a high order—not, however, so high as might at first sight be inferred from Professor Maxwell's statement that, for the case of  $i$  factors, it depends on the solution of a system of  $2i$  equations of the  $i$ th degree, as the equations referred to (evidently those obtained by the use of the method of indeterminate coefficients in its crude form) would be of a special character: thus, *ex. gr.*, when  $i=2$ , the order of the system of the four quadratic equations sinks down from  $4 \cdot 2^3$  or 32 (its value in the general case) to be only 3, as will presently be seen.

The method of poles for representing spherico-harmonics, devised or developed by Professor Maxwell, really amounts to neither more nor less than the choice of an apt canonical form for a ternary quantic, subject to the condition that the sum of the squares of its variables (here differential operators) is zero; and I am quite at a loss to understand how it can at all assist "in making the conception of the general spherical harmonic of an integral degree perfectly definite," or what want of definiteness apart from the use of this canonical form can be said to exist in the subject.

Since  $\left(\frac{d}{dx}\right)^2 + \left(\frac{d}{dy}\right)^2 + \left(\frac{d}{dz}\right)^2$  retains its form when any orthogonal linear substitutions are impressed on  $x, y, z$ , we recognize *a priori* that a harmonic distribution on the surface of a sphere is invariantive in the sense that it bears no intrinsic relation to the particular set of axes which may happen to be used to express the value of the harmonic at each point of the surface; and the great merit, it seems to me, of Professor Maxwell's beautiful conception of harmonic poles is that it puts this fact in evidence: for it is easy to see at a glance, from the use of successive linear operators, that the harmonic at any variable point on the surface for any given degree ( $n$ ) will depend in an absolutely determinate manner (save as to an arbitrary constant factor) on the cosines of the arcs joining it with  $n$  arbitrarily assumed fixed points on the sphere, and of the arcs joining those  $n$  points with one another (being in fact a symmetrical function of each of the two sets of cosines), so that intrinsic poles are substituted for extrinsic Cartesian axes. I am a little surprised that this distinguished writer should not have noticed that there is always one, and only one, *real* system of poles appertaining to any given harmonic, and that to find this system it is not necessary, as he has stated, to employ a system of  $n$  equations each of the order  $2n$ , but one single equation of that order. For calling  $\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$  by the names  $\xi, \eta, \zeta$ , then any given harmonic of the  $n$ th degree may be re-

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duced by the use of mere linear equations to the form  $(\xi, \eta, \zeta)^n \frac{1}{r}$ , and the problem to be solved in order to find its poles is the purely algebraical one of converting the quantie

$$(\xi, \eta, \zeta)^n + \Lambda(\xi^2 + \eta^2 + \zeta^2),$$

where  $\Lambda$  is a quantie of the order  $(n-2)$ , into a product of linear factors. Now this again is merely the problem of finding a pencil of rays that shall pass through the intersections of the curve  $(\xi, \eta, \zeta)^n$  with the curves  $(\xi^2 + \eta^2 + \zeta^2)$ ; that is to say, any dispersal of the  $2n$  intersections into  $n$  sets of two each will give a system of  $n$  polar factors in Professor Maxwell's problem. We have therefore only to find the values of  $\xi : \eta : \zeta$  in the two simultaneous equations  $(\xi, \eta, \zeta)^n = 0$ ,  $\xi^2 + \eta^2 + \zeta^2 = 0$ , and this leads to a resolving equation of the  $2n$ th order. From the form of the second equation we see that the values  $x : y : z$  are *all imaginary*; consequently there will be one, and but one, system of real rays, *i. e.* real polars corresponding to the distribution of the  $2n$  roots of the resolving equation into  $n$  *conjugate pairs*. The remaining systems (there are in all  $1.3.5 \dots 2n-1$  of them) will each contain imaginary elements, so that all or some of the poles become imaginary.

In the case of  $n=2$ , the problem becomes the familiar one of finding the principal axes of a cone of the second order; and instead of employing a biquadratic resolvent we make the discriminant of  $(\xi, \eta, \zeta)^2 + (\xi^2 + \eta^2 + \zeta^2)$  to vanish, which of course only requires the solution of a cubic equation; but as subsequently (when the pair is to be divided into its elements) a new quadratic surd is introduced, we are virtually solving a biquadratic, in accordance with the general rule that, to find the poles of a spherical harmonic of the degree  $n$ , it is necessary to solve an equation of the degree  $2n$ .

To put the coping-stone to Professor Clerk Maxwell's method of poles, I think it would be desirable to find an intrinsic definition of spherical harmonics to correspond with their representation referred to intrinsic axes: I mean we ought to be able to dispense with the Laplacian operator altogether, and to define a Harmonic with sole reference to some algebraical or geometrical (but certainly not physical) condition which it satisfies in regard to its poles. With all possible respect for Professor Maxwell's great ability, I must own that to deduce purely analytical properties of spherical harmonics, as he has done, from "Green's theorem" and the "principle of potential energy" (*Electricity and Magnetism*, vol. i. p. 168), seems to me a proceeding at variance with sound method, and of the same kind and as reasonable as if one should set about to deduce the bi-

nomial theorem from the law of virtual velocities, or make the rule for the extraction of the square root flow as a consequence from Archimedes' law of floating bodies.

Johns Hopkins University,  
Baltimore, Maryland, U. S.,  
August 28, 1876.

### XXXVIII. *Notices respecting New Books.*

*Tables for facilitating Sumner's Method at Sea.* By Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S., Professor of Natural Philosophy in the University of Glasgow, and Fellow of St. Peter's College, Cambridge. London: Taylor and Francis, 1876.

IN 1871 Sir William Thomson communicated to the Royal Society a paper which was published in its 'Proceedings,' in which, after stating his decided preference for Sumner's over any other method of reducing observations at sea, he went on to show how Tables might be prepared which would greatly shorten and simplify the application of that method. He has had these Tables calculated under the superintendence of Mr. Roberts, of the Nautical-Almanac Office; and they have been largely used by the author and his friends during several years. These private trials have served to prove the practical utility of Sir W. Thomson's system, and to justify its publication. The Tables are now issued, accompanied by Rules for their use, which are sufficiently explicit to direct the sailor, even although he should be ignorant of the principles of the method he is applying. The method, however, is fully explained in a preface to the book, which is of considerable scientific interest, and of which we proceed to give a short abstract.

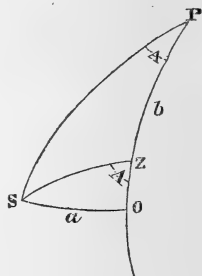
Sir W. Thomson begins by pointing out that any single observation of the altitude of the sun or of a star at a known instant of Greenwich time affords data for drawing on the earth's surface a circle, at some point in which the observer must have been, and from every point in which the altitude of the body observed was the same at the time of the sight. Of course the body observed must at that time have been vertically overhead of the centre of this imaginary circle, and every part of the circle is perpendicular to the true bearing of the body observed. All that any one "sight" tells the sailor is that the ship is somewhere upon one such circle; and if he can actually draw a small portion of the circle (short enough to be taken without sensible error as a straight line) in that part of his working chart in which the ship is known to be, he will have obtained all the information that the "sight" can yield, and he will have expressed it in the most concise and scientific form possible. The short straight portion of this circle, which it is the object of Sumner's method to draw, is commonly called a Sumner line.

When an observation of time and altitude has been made, the corresponding Sumner line may be drawn in either of two ways. We may take two estimated latitudes, say the two integer degrees nearest to the true latitude, and calculate the longitude corre-

sponding to each. If the two points thus found be joined, the line joining them will be the Sumner line. Or we may calculate one longitude (and so obtain one point through which the line passes), and also the true azimuth of the body observed, which gives us the inclination of the Sumner line to the parallels of latitude. The latter plan is the one adopted by Sir W. Thomson. The former is the plan most commonly used, although neither can properly be said to be in common use at all; for, as Sir W. Thomson says,

“Sumner’s method, although undoubtedly the clearest and completest mode of interpreting observations, has never found very general favour with navigators. A few of the most skilful no doubt use it habitually; and many more use it occasionally in critical times; but its tediousness has hitherto prevented it from coming into every-day use. I have prepared the Tables which follow with a view to remove this drawback to the use of a method which I have long felt convinced should be the rule and not the exception of practice at sea.”

When the latitude is estimated, the problem of how to find the hour-angle and azimuth is simply the solution of the spherical triangle  $PSZ$ , in which  $P$  is the earth’s pole,  $Z$  the ship’s zenith, and  $S$  the sun or star. The data are  $PZ$  (the estimated co-latitude),  $SZ$  (the zenith distance or co-altitude), and  $SP$  (the polar distance or co-declination). It is required to find the angle  $SPZ$ , which is the hour-angle from the ship’s meridian, and  $SZO$ , which is the azimuth. Sir W. Thomson shows that to tabulate the values of the angle  $P$  corresponding to all possible values (differing by not more than  $1'$ ) of the three sides of the triangle would be impracticable, as it would necessitate the tabulation of the solution of 157,464,000,000 triangles, which at the rate of 1000 triangles per day would take 400,000 years. He has overcome this formidable difficulty by dividing the problem into the solution of two right-angled spherical triangles, and by aid of the tabulated solutions of 8100 triangles whose sides are integral numbers of degrees, has made it possible and easy to work out a ship’s place as accurately as the observations admit of, with hardly any calculation, and without the use of logarithms. The account of how this has been done is given in the preface with a conciseness which hardly admits of abbreviation:—



“Let  $O$  be the point in which the arc of a great circle less than  $90^\circ$ , through  $S$ , perpendicular to  $PZ$ , meets  $PZ$ , or  $PZ$  produced. We have now the two right-angled spherical triangles  $SPO$  and  $SZO$ , which have the common side  $SO$  or  $a$ , whose angles  $A$  opposite to  $a$  are in the one the hour-angle, and in the other the azimuth, whose sides  $b$  ( $OP$  and  $OZ$ ) differ by an amount equal to the estimated co-latitude  $PZ$ , and whose hypotenuses ( $SP$  and  $SZ$ ) are the co-declination and co-altitude respectively.

“The side  $a$  may have any value from  $0^\circ$  up to  $90^\circ$ . The side

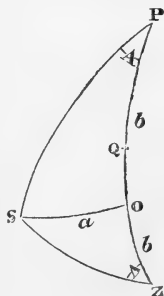


$b$  may also have any value from  $0^\circ$  to  $90^\circ$ . The Tables are calculated for the successive values of  $a$  and  $b$ , increasing by  $1^\circ$  at a time. Instead of the values of the hypotenuses (SP and SZ) being tabulated, the *complements* of their values are tabulated, so as to give directly the declination and altitude instead of the co-declination and co-altitude. These quantities are given in the columns headed 'co-hyp.' The values of the angles opposite  $a$  are given in the columns headed 'A.'

"When we have made an observation and estimated our latitude to the nearest degree, we do not know the value of  $b$  for either triangle; but we know what the difference between the values of  $b$  for the two triangles is, it being equal to the estimated co-latitude. We do not know the value of  $a$ ; but we know the values of the two co-hypotenuses, the complements of SP and SZ, since they are equal to the declination and altitude respectively. We therefore take the Tables and search from column to column until we find two places in one of the columns headed ' $b$ ,' distant from each other by an amount equal to the estimated co-latitude, and which are such that opposite to one of them in the adjoining column headed 'co-hyp' is a number approximately agreeing with the declination, while opposite to the other (and also in the 'co-hyp' column) is a number approximately agreeing with the altitude. We then take from the adjoining column headed 'A' the number level with the declination for the hour-angle, and that level with the altitude for the azimuth. In fact we have two level sets of numbers in the three columns ' $b$ ,' 'co-hyp,' and 'A,' one set distant from the other by a length of the column ' $b$ ' equal to the estimated co-latitude, and which give the solutions of the two triangles SPZ and SZO. . . . The search from column to column until the proper places are found, is, with the help of a pair of compasses, a very simple matter. It will almost always happen that the numbers found in the 'co-hyp' column agree only approximately with the actual declination and altitude. It is then necessary to get a second set, such that the actual value of the declination will lie between the two tabular values, and then to interpolate intermediate values for the altitude, hour-angle, and azimuth corresponding to the actual declination."

In cases where the point O comes between P and Z:—

"The two triangles SPO and SZO will, as before, have the common side  $a$ ; their co-hypotenuses will, as before, be the declination and altitude respectively; and their angles A (SPO and SZO) will be the hour-angle and azimuth respectively. The side  $b$  of the one will, however, no longer differ from the side  $b$  of the other by an amount equal to the estimated co-latitude PZ; in place of this relation the following one will exist. Let PZ be bisected in Q; then PO (the side  $b$  of the one triangle) = PQ + OQ =  $\frac{1}{2}$ PZ + OQ, while OZ (the side  $b$  of the other triangle) = QZ - OQ =  $\frac{1}{2}$ PZ - OQ. In other words, the side  $b$  in the one triangle is just as much *greater*



than the half estimated co-latitude as the side  $b$  in the other triangle is *less* than the half estimated co-latitude. We therefore take the Tables and find, by aid of a pair of compasses, two places somewhere in one of the columns headed ' $b$ ,' one of which is as much above the level of the half estimated co-latitude as the other is below that level, and such as to have two numbers opposite to them in the 'co-hyp' column approximately agreeing with the declination and altitude respectively. The numbers level with these in the adjoining column ' $A$ ' will then agree with the hour-angle and azimuth."

In the Rules which are appended to the Tables a full account is given of how to find the exact position by means of the intersection of two Sumner lines, or of one Sumner line with the true bearing of an object whose position is known.

Believing as we do that Sumner's is the only scientific method of treating observations of the sun and stars at sea, and that its general adoption by sailors is a reform much to be desired, not only because of the practical merits of the method, but also, and perhaps chiefly, on account of its leading those who use it to inquire into the reasons of what they do, we welcome this volume as forming an important addition to the already heavy debt which the scientific navigator owes to Sir William Thomson.

*Railway Appliances A Description of details of Railway Construction subsequent to the completion of the Earthworks and Structures including a short notice of Railway Rolling Stock* By JOHN WOLFE BARRY Member of the Institution of Civil Engineers With Illustrations. London: Longmans, Green & Co. Small 8vo, pp. 299. (Text-Books of Science.)

This work can hardly be regarded as a "Text-Book of Science;" and it occurs oddly enough in a series which is designed—amongst other ends—for the use of boys in Public Schools. However, that is but a small matter; for it is a book which (we doubt not) will greatly interest the reading public. We all travel by railway, and see, but by no means understand, a variety of mechanical contrivances used in connexion with locomotion—signals, of which we only know that they are intended to convey information to somebody—handles, of which we only know that they are to be pulled.

Mr. Barry comes in and enlightens us on all these points; tells us how the permanent way is formed; describes the various kinds of sleepers, chairs, and rails, and the ways of fastening them together; discourses of points and crossings, signals, and train-describers, turn tables, and traversers. He discourses of stations and their varieties, whether terminal or roadside, whether for passengers or for goods, and fully sets forth every contrivance connected with them. He shows how those familiar objects the Paddington, Euston, and Cannon-Street Stations illustrate the general principles of railway arrangement; all three are good, in his opinion, though, if we may hazard a conjecture, Cannon-Street is the station he loves best.

Often as the reader must have been at that famous station, the following particulars will probably be new to him. The minimum

width, says our author, to be given to platforms of terminal stations admits of much difference of opinion. At Cannon-Street, where land was extremely expensive, and every dimension was carefully apportioned, "the platform area is probably minimized; but the station accommodates efficiently a very large mixed traffic of long- and short-journey trains, amounting at times to as many as 400 trains in and 400 trains out in a working day." To accommodate this enormous amount of traffic, there are an eastern and a western platform, each being  $13\frac{1}{2}$  ft. wide, and having a line of rails on both sides, the former being 522 ft., the latter 486 ft. long. These are for local traffic; the general departure platform for main-line traffic has a line of rails on each side, and is 19 ft. wide and 665 ft. long; the general arrival platform is 721 ft. long, has a cab-road through the middle, and a line of rails on both sides—as appears from the plan (p. 194); but possibly only one side is commonly used, as our author describes it as being 12 ft. wide opposite the cab-road, 30 ft. wide beyond it, and as having a line of rails on one side only.

The chapters on signals and on the block system are those to which the general reader will be likely to turn with most interest; these bear most immediately on the question will the journey be safely made or not? On this head Mr. Barry has good tidings for him. Like a practical man, it is true, our author scouts the notion of absolute safety for the passenger; *that* would be inconsistent with the interests of the company: "if every possible known precaution is to be taken, regardless of cost, it may not pay to work a railway at all" (p. 179). But short of this a great deal can be done. A nervous traveller, who has noticed the rails and points near Cannon-Street, will take courage when he reads the following statement. There are, it is true, seventy point and signal levers capable of being combined in many millions of different ways, all dangerous except 808 combinations which are safe; all the levers, however, are brought into a single house and locked together in such a manner that no combination but one of the 808 can possibly be made. "If a man were to go blindfold into a signal-box with an interlocking apparatus, he might, so far as accordance between points and signals is concerned, be allowed with safety to pull over any lever at random. He might doubtless delay the traffic, because he might not know which signal to lower for a particular train, but he could not lower such a signal, or produce such a combination of position of points and signals, as would, if the signals were obeyed, produce a collision. The results of the interlocking principle may be illustrated by the example of a piano or organ, constructed in such a way that no notes could be played on it which are not in harmony with each other" (p. 113).

Mr. Barry gives a lucid account of the Block System—a name which the system will doubtless retain, though it is not very appropriate and its origin is doubtful,—and of the modifications by which its signals are adapted to different circumstances. Thus at a roadside station bell-signals may be sufficient; but these would not do at a terminus where, as at Cannon-Street, a large number

of (say twelve) kinds of trains have to be described by signal. In such a case an elaborate instrument called the train-describer has to be used (p. 158). As well as a description of the Block System, Mr. Barry gives a discussion of the advantages which it possesses over other systems; and his conclusion is as follows:—"Viewing the block system as a whole, it may be safely said that it has the advantage of being perfect in principle in so far that it renders collisions impossible provided it be carried out perfectly; whereas the system of time signalling, even granting that it can be carried into effect without a flaw, gives no absolute security against collisions" (p. 165).

Perhaps the point which strikes us most forcibly when reading such a book as that before us, is the enormous amount of ingenuity that has been brought to bear in working out the details of our present system of locomotion. The need for a large part of the contrivances actually in use could not possibly have been foreseen; it could only have come into view as the system was being gradually worked out. Of course, therefore, further improvement is still possible; and if by any chance we have a railway manager among our readers, we would earnestly recommend to his attention the following suggestion. "Another great improvement would be in paying more attention to distinguishing the trains, or, where necessary separate carriages, by boards on which their destination should be legibly painted, placed in positions on the carriage where the boards would be visible when the train is standing at the platform. This is particularly necessary on the metropolitan and suburban lines; but the notice boards on the carriages are usually very indistinct, as if railway managers were ashamed of them; and they are often so placed that they cannot be seen well at any time and scarcely at all at night" (p. 280).

*Exercises in Electrical and Magnetic Measurement, with Answers.* By R. E. DAY, M.A. London: Longmans, Green & Co., 1876 (small 8vo, pp. 120).

This book contains an introduction of four pages on the units employed in Electrical Measurements, one hundred and twelve pages of questions, and an appendix of answers. The questions are about 650 in number, and are given barely, without any note or comment. The answers, with very few exceptions, are numerical. The questions have not been framed as exercises for the ingenuity of the student, but are, as the author states, short descriptions of experiments illustrating the fundamental laws of electricity and magnetism, and are for the most part based upon experimental work actually performed in the laboratory at King's College, London. A single question, taken almost at random as a specimen, will show what the book is better than a long description:—"A battery of 10 similar galvanic cells was joined up in circuit with a voltmeter, and it was found that 80 cubic centimetres of hydrogen were liberated per minute. When an additional resistance of 15 ohms was introduced into the circuit, the volume of hydrogen liberated per minute was reduced to 50 cubic centimetres.

Find the electromotive force of one cell of the battery" (p. 67). It will be well to add that in the problems the author has "almost exclusively employed the nomenclature and the system of units recommended by the Committee of the British Association."

### XXXIX. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 233.]

March 23, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following paper was read:—

"On the Nature of the Force producing the Motion of a Body exposed to Rays of Heat and Light." By Arthur Schuster, Ph.D., Demonstrator in the Physical Laboratory of Owens College.

Mr. Crookes has lately drawn attention to the mechanical action of a source of light on delicately suspended bodies *in vacuo*; I have made a few experiments which will, I think, throw some light on the cause of the phenomenon, and assist us in the explanation of the manifold and striking experiments made by Mr. Crookes.

Whenever we observe a force tending to drive a body in a certain direction, we are sure to find a force equal in amount acting in the opposite direction on the body from which the force emanates. It was with the view of finding the seat of this reaction that I have made a few experiments.

If the force is directly due to radiation, the reaction will be on the radiating body; if, on the other hand, it is due to any interior action, such as the one suggested by Prof. Reynolds, the reaction will be on the enclosure of the moving bodies. I have been able to test this by experiment, and I have found that the action and reaction is entirely between the light bodies suspended *in vacuo* and the exhausted vessel.

The instrument best fitted for an experimental investigation of this kind is the one which has been called "radiometer" by Mr. Crookes. These instruments have been made in great perfection by Dr. Geissler, of Bonn, under the name of "light-mills." Thanks to the courtesy of Prof. Reynolds, I have been enabled to work with such an instrument. The "light-mill" was suspended by means of two cocoon fibres, forming a bifilar suspension, from the top of a vessel which could be exhausted. A slight movement of the enclosure could be easily detected by means of a concave mirror attached to it. A beam of the oxyhydrogen lamp was concentrated on the light-mill, which then revolved about 200 times a minute.

The light was cut off at the beginning of the experiment by means of a screen, and the position of rest of the glass vessel was read off by means of the dot of light on the scale. The screen was then suddenly removed, and in every case a large deflection of the glass vessel was observed. The vessel was deflected in the opposite direction to that in which the mill turned. When the velocity of

the mill had become constant, the vessel returned to its original position. On suddenly cutting off the light the vessel was again deflected, but in the opposite direction to that on starting the experiment. The vessel therefore now turned in the same direction in which the mill turned.

These experiments are easily explained on the assumption that the force acting on the vessel enclosing the light-mill is exactly equal and opposite to that acting on the mill itself. While the velocity of the mill in one direction is increasing, a force acts in the opposite direction on the vessel. When the velocity has become constant, the force which tends to drive the mill round is exactly counterbalanced by the resistance which opposes the motion of the mill. The two forces acting on the vessel will therefore counterbalance, and the vessel will return to its original position of rest. When the light is cut off, the resistance will stop the motion of the mill. The reaction of the resistance will act on the enclosure, and the enclosure will turn in the same direction as the mill.

By means of the reaction on the enclosure I have been able to calculate the strength of the force; and I have found that the pressure on a surface on which light of equal intensity to that used in my experiments falls, is equal to that produced by the weight of a film of water on a horizontal surface equal in thickness to the length of a wave of violet light.

March 30.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

The following paper was read:—

“The Residual Charge of the Leyden Jar.” By J. Hopkinson, M.A., D.Sc.

1. If it be assumed that a dielectric under electric induction has every element of volume of its substance in an electropolar state, and also that dielectrics have a property analogous to coercive force in magnetism whereby time is requisite for the development or decay of this electropolar state, an explanation of the residual charge of the Leyden jar easily follows. Adding the hypothesis, which at first sight appears most probable to connect the induction and polarization with the time by two differential equations, it follows that the potential  $E$  of a Leyden jar when insulated may be expressed in the form  $E = (A + B e^{-\mu t}) e^{-\lambda t}$ , where  $\lambda$  and  $\mu$  are constants for the material, and  $A$  and  $B$  depend on the previous states of the dielectric.

2. Observations made with the quadrant electrometer, the condenser being a Florence flask containing sulphuric acid, shows that  $E$  cannot be so expressed. Glass is a mixture of different silicates, and it may be supposed that each substance is capable of independently being electropolar; there will thus be  $E$  and more than one polarity to be connected with the time by more than two differential equations. Making a similar obvious hypothesis regarding these relations,  $E$  must be expressed in the form  $\sum_0^r A_r e^{-\lambda_r t}$ . If this be so, it would probably be possible to charge a Leyden jar in such a

way that the sign of the return charge after discharge and insulation should change once or more.

3. This anticipation was verified by charging the flask alternately positively and negatively for successively decreasing periods. The author learned after making this verification that Sir William Thomson had tried similar experiments before, but had never formally published them.

4. The analogy between coercive force in magnetism and the electro-coercive force suggested that, as mechanical agitation shakes out the magnetism from a magnet, so it might shake down the electropolar state of a dielectric and unmask residual charge more rapidly than is the case in quiescence. This was found to be the case; a residual charge manifests itself in the flask more rapidly when the flask is tapped than when it is quiet. It was also found that that portion of the return charge which comes out last is more accelerated by vibration than that which comes out first, and that, after tapping, the flask was less susceptible to the effect of tapping than it was before it was touched.

5. Experiment shows that, after a return charge has attained a maximum and is decreasing by conduction through the glass, the loss per cent. per unit of time does not continuously increase from zero at the point of maximum potential but may presently decrease.

6. Sir William Thomson explained specific inductive capacity by supposing every part of the dielectric to be electropolar under induction; by introducing time into that explanation, it is made to cover both specific inductive capacity and that on which residual charge depends as respectively rapid and slow cases of similar phenomena.

#### GEOLOGICAL SOCIETY.

[Continued from p. 153.]

June 21st, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. “On the Ice-Fjords of North Greenland and on the formation of Fjords, Lakes, and Cirques in Norway and Greenland.” By M. A. Helland.

The author described in great detail his observations on the glacial phenomena of Greenland, and applied their results to the consideration of the traces of glacial action exhibited in Norway. He stated that the glacier of the Jacobshavn fjords, moving on a slope of only  $\frac{1}{2}^{\circ}$ , advances, at all events in summer, at from 14·20 to 19·77 metres per day, the maximum movement being about 64 feet per day. In all cases where the glaciers terminate in fjords, and the formation of the latter is due to glacial action, it is found that the mouth of the fjord is shallow, with an island or islands bearing erratic blocks, whilst the interior is much deeper even than the sea outside. The same characters are observed in the

Norwegian fjords. From a consideration of the height at which these boulders occur on islands at the mouth of the latter, the author estimates the thickness of some of the glaciers, to which he attributes the formation of the fjords, and arrives at thicknesses varying from 2940 to 7010 feet. The author remarks that fjords, lakes, and cirques are always met with in glaciated regions, and explains their formation by the action of ice. His view of the course of events in Norway is as follows:—Before the Glacial epoch thousands of streams commenced the work of erosion and produced valleys. During the Glacial epoch these valleys were enlarged and lake-basins were hollowed out. The descending glaciers ground out fjords to their full length when the Glacial epoch was at its highest; but as it declined the glaciers ground out the inner part to a still greater depth, producing the present characters of the marine fjords, and giving rise to lake-hollows in other places. That the glaciers once extended beyond the fjords is shown by moraine-matter being dredged up. Some of the sea-banks and islands off Christiania-fjord are old moraines; and if Norway should be raised 400 metres, these banks would show as moraines and plains before the lake-basins of the fjords.

2. "On the Drift of Brazil." By C. Lloyd Morgan, Esq., F.G.S., Assoc.R.S.M.

The author described the position and mode of occurrence of large boulders of gneiss and granite in the red drift of Brazil and on the slopes of hills even at considerable elevations, and stated that, like Professor Agassiz, he could not see how these could have been transported to their present positions except by the agency of ice. At the same time he stated that none of these boulders exhibit any glacial scratches, nor are any such markings perceptible on exposed rock-surfaces; their absence on the latter he thought might be due to weathering. The surface of the gneiss on which the red drift lies, however, is *moutonnée*. The author remarked with regard to the great glacier which Prof. Agassiz imagined to have filled the valley of the Amazons, that the Andes from which he supposed it to have come, are more than 1500 miles from Rio, and that right across the country there stretches an almost continuous series of mountains and hills. He also called attention to the existence of mountains in eastern Brazil of sufficient height to have nourished glaciers, but at the same time remarked that the question of the origin of the supposed glaciers cannot be satisfactorily answered until the boulders found in the clay have been traced to their sources. For his own part he is inclined to believe that the drift, if of glacial origin, was not formed by glaciers taking their rise in any of the peaks indicated by him, but by an almost universal South-American ice-sheet.



XL. *Intelligence and Miscellaneous Articles.*

ON THE PHOTOMETRY OF COLOURED FLAMES. BY M. GOUY.

THE photometric process of which the description forms the subject of this Note permits us to measure with facility the brightness of the various lines constituting the spectrum of coloured flames.

When a source of light is examined with the spectroscope, a spectrum is seen of which the brightness varies according to the point considered, the nature of the source, and the width of the slit. The luminous intensity in a point of the spectrum is expressed by the integral  $k \int_{\lambda_1}^{\lambda_2} i d\lambda$ .

In this formula,  $i$  is the intensity of the ray whose wave-length is  $\lambda$ , emitted by the source;  $k$  is a coefficient depending on the construction of the apparatus and the loss of light by absorption and reflection;  $\lambda_1$  and  $\lambda_2$  are two wave-lengths such that a source which emitted only those two rays would give in the spectroscope two lines in contact whose common margin would pass through the point considered.

If we project upon the slit of the spectroscope, supposed vertical, the image of a Babinet's compensator arranged so as to give horizontal fringes, illuminating it by two luminous pencils polarized at a right angle and emitted by two different sources, a spectrum will be produced streaked with horizontal fringes. Each of the sources will give a system of fringes; and the dark fringes of the one system being superposed to the bright ones of the other, the portions of the spectrum in which the preceding integral has the same value for both sources will alone preserve their usual aspect. By varying in a known ratio the intensity of the light emitted by one of the sources, we shall be able to measure the brightness of the different parts of the spectrum which it produces. If it gives a line-spectrum, the slit is to be opened sufficiently for the lines to become broad bands; this will make the observation easier.

The apparatus is arranged thus:—In front of the slit of a spectroscope is placed the photometer, which comprises the following pieces, ranged in a straight line in the axis of the collimator:—an achromatic lens; a Nicol with its principal section horizontal; a piece similar to a Babinet compensator, in which the axes of the quartz prisms are at  $45^\circ$  from the fringes, which are horizontal; an achromatized spar prism, of which the principal section is horizontal; a Nicol movable at the centre of a graduated circle; and a lens.

Between the spar prism and the last Nicol the tube carries a lateral branch at a right angle, which contains a total-reflection prism and a lens. A flame being placed before this lateral branch, the light which is not stopped by the diaphragms passes through the spar as the extraordinary ray; the contrary takes place for a flame placed before the movable Nicol. The two flames thus give complementary fringes, the image of which is thrown upon the slit of the spectroscope and passes into the spectrum.

By putting a lamp in the place of the slit of the spectroscope and following the rays which have passed through the photometer, one can see what points of the space can send light into the spectroscope, and consequently regulate the apparatus. These rays form a condensed pencil with a diameter of less than 5 millims. and length of 20 centims.

The flames which I employed were produced in the manner following. Air compressed to 1.5 atmosphere issues through an adjutage, above which is fixed the tapering end of a glass tube, the other extremity of which dips into a saline solution; the solution is aspirated and reduced to a very fine powder. The illuminating gas is aspirated at the same time, mixes with the air and the saline powder, and burns above a metallic gauze. The blue part of the flame (which gives the lines of carbon) rises 2 or 3 millims. above the gauze; the flame is afterwards homogeneous and but little reducing; it forms a cone 8 centims. in height by 2 in diameter. It was towards the base that my observations were made in the experiments, of which I have now to speak.

The object of these was to seek the mean intensity of the rays which constitute the lines of the spectrum of coloured flames. Let  $k$  be a constant,  $i$  the intensity of the ray of wave-length  $\lambda$ ,  $\lambda_1$  and  $\lambda_2$  two wave-lengths selected on one side and on the other of the line considered, the brightness of this line will be equal to

$$I = k \int_{\lambda_1}^{\lambda_2} i d\lambda.$$

If I place near the flame a mirror of reflecting-power  $R$ , arranged so that the same part of the flame sends its light into the photometer direct and after reflection, the reflected rays will meet with the flame at the place to which they have been emitted; the brightness of the line becomes

$$I' = (1 + R)k \int_{\lambda_1}^{\lambda_2} i d\lambda - Rk \int_{\lambda_1}^{\lambda_2} i^2 d\lambda,$$

supposing that the unit chosen was the intensity of the ray of the same wave-length, emitted by a substance with absolute absorptive power and raised to the temperature of the flame, and applying the principle of the equality of the powers of absorption and emission, whence

$$\frac{I(1 + R) - I'}{2RI} = \frac{1}{2} \frac{\int_{\lambda_1}^{\lambda_2} i^2 d\lambda}{\int_{\lambda_1}^{\lambda_2} i d\lambda};$$

this is the ordinate of the centre of gravity of the area included between the axis of the  $\lambda$ 's and the curve formed by erecting at each point of this axis an ordinate equal to  $i$ . This height of the centre of gravity may vary from 0 to  $\frac{1}{2}$ ; and its value indicates which are the dominant radiations.

The experiments have shown that the narrow lines (sodium, thallium, lithium, the blue line of strontium) have a centre of gravity

elevated and near the ordinate 0·3. Its height increases but little with the brightness of the flame.

The following are the numbers found for lithium :—

Brightness.	Height,
6·13 .....	0·29
4·6 .....	0·285
2·6 .....	0·275

For sodium the brightness varied from 1 to 15, and the height from 0·28 to 0·30. It is seen that the augmentation of the brightness is effected chiefly by the widening of the lines.

On the other hand, the nebulous lines ( $\alpha$ ,  $\gamma$ , and  $\delta$  of strontium,  $\alpha$  and  $\beta$  of calcium) have a centre of gravity of which the height is comprised between 0 and 0·03. It is probable that the centre of gravity rises when the brightness is increased; but my experiments are too incomplete to enable me to affirm any thing on this point. — *Comptes Rendus de l'Académie des Sciences*, vol. lxxxiii. pp. 269–272.

#### NEW MINERALS.

Professor C. U. Shepard has named and partially described the following new species :—

*Vanuxemite*.—Occurs in small irregular patches scattered through a firm ochry aggregate, proceeding from the decomposition of several zinc ores. Massive, impalpable, with an even or conchoidal fracture. Colour white, dull. H. = 2·5–3, G. = 2·5. Does not adhere to the tongue, but emits a feeble clayey odour on being breathed upon. B. B. fuses readily to an opaque enamel. Composition  $\text{SiO}_2$  35·64,  $\text{AlO}_3$  11·70,  $\text{ZnO}$  32·48–36,  $\text{H}_2\text{O}$  14·80–19·88 = 99·70. Locality, Sterling Hill, N. J.

*Keatingine*.—Considered “probably a new species.” Closely resembles Fowlerite in crystalline structure, but angles obtained on cleavage prisms  $64^\circ$  and  $116^\circ$ . Does not lose lustre on weathering. H. = 4·5–5, G. = 3·33. B. B. fuses to a reddish semitransparent glass. Composition  $\text{SiO}_2$  47·8,  $\text{MnO}$  27·7,  $\text{ZnO}$  5·6,  $\text{CaO}$  18·0,  $\text{H}_2\text{O}$  0·8 = 99·9. Locality, Franklin, N. J., where it was found in a mass of yellow garnet.

*Calcozincite*.—Massive, fine-granular; interpenetrated with fibres of asbestos and Sussexite. Lustre vitreous. Colour light orange-red. Streak lemon-yellow. Translucent. H. = 3·5, G. = 3·95. Effervesces slightly with acid. B. B. blackens. Composition  $\text{ZnO}$  81·00,  $\text{CaO}$  7·56,  $\text{CO}_2$  5·80,  $\text{H}_2\text{O}$  4·26,  $\text{MnO}$  tr = 98·62.

*Euchlorite*.—Massive, in coarse elongated scales. Colour light olive-green. Powder pale green. Lustre subpearly. H. = 2·5–3, G. = 2·71. B. B. fuses with difficulty on thin edges to a greenish-grey enamel. Decomposed by sulphuric acid. Composition  $\text{SiO}_2$  35·51–38·46,  $\text{AlO}_3$  6·80,  $\text{FeO}$  15·52, ( $\text{MgO}$  38·07),  $\text{H}_2\text{O}$  6·10, 100. Locality, Chester, Mass., where it occurs in a layer on both sides of an extensive vein of albite.

*Pelhamine*.—Forms irregular seams and masses sometimes a foot thick at the asbestos-mine at Pelham, Mass. Resembles a black serpentine closely. Almost without lustre. Powder dark greenish grey. H. = 5·0, G. = 2·9–3·2. B. B. infusible. Composition

SiO<sub>2</sub> 38.40, AlO<sub>3</sub> 2.80, FeO 15.52, (MgO 39.88,) H<sub>2</sub>O 3.40=100.  
—Silliman's *American Journal*, September 1876.

ON THE INFLUENCE OF SONOROUS VIBRATIONS UPON THE  
RADIOMETER. BY J. JEANNEL.

I have ascertained that the rotatory movements of the radiometer can be determined by certain sonorous vibrations. In regard to this I have, in conjunction with M. Coulier and M. Alvergniat, made various experiments, of which I think I ought to render an account to the Academy.

In semiobscurity three radiometers A, B, C, of unequal sensitiveness and at perfect rest, were placed on the inner shelf of a saloon-organ. The bass notes (those of the first three octaves) determined the rotation-movements. The deepest bass notes have the greatest action; nevertheless the *fa* and the *fa* sharp of the lower octave, especially with the bourdon stop, produce the most rapid rotation. *Ut*, *re*, and *mi*, though deeper, have much less action.

The radiometers do not all behave in the same way as regards the speed and the direction of their rotations. Thus, through the effect of *fa* or *fa* sharp of the lower octave, radiometer A (the least sensitive to light) rotates with the velocity of about one turn per second, with the black faces of the vanes in front—that is to say, the inverse of the motion produced by light; B and C (more sensitive to light) turn more slowly, and in the direction of the light-produced motion, viz. with the bright faces in front.

These facts I propose to explain thus. As certain notes produce no effect, it is evident that the needle which forms the internal support of the revolving arms must be able to vibrate in unison with the notes of the organ in order that the rotatory movement may be produced. Certain vibrations of the organ-shelf, transmitted to the needle, communicate to it circular or angular vibrations, from which results the rotation of the arms which it supports. What appears to demonstrate that this explanation is well founded is, that on pressing the soft part of the finger upon the top of the radiometer we prevent it from vibrating, and, at the same time, from turning.

The shelf of a piano produces similar effects, but in a less degree.

When the above experiments are tried in a medium where the diffused light is nearly sufficient to start the radiometer, even the feeblest deep sounds determine the rotation in the usual direction: the roll of the vehicles is sufficient.

Here the explanation presents itself to the mind spontaneously. The arms undergo the impulsion of the force which is to put them in motion; but this force is counterbalanced by the friction of the top piece upon the point of the needle; no motion can take place. But if the vibrations interpose, the top piece, raised above the needle-point and thus removed from friction during a certain space of time, can obey the impulse. Hence it results that under the influence of the vibrations the radiometer appears much more sensitive to light than when it is at rest.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxiii. p. 445.

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[FIFTH SERIES.]

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NOVEMBER 1876.

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XLI. *On a Method of Measuring the Contour of Electric Waves passing through Telegraph Lines.* By ROBERT SABINE\*.

VARIOUS methods have during the last forty years been suggested and put in practice with the view of ascertaining the time which elapses between starting electricity into one end of a conductor, and the instant when the advancing wave acquires strength to do the work necessary to give some indication at the other end.

Sir William Thomson, in his admirable paper on the "Theory of the Electric Telegraph," has clearly shown that, in any form of conductor with any given receiver, the time required for a wave to give a signal is dependent upon the induction and the resistance of the line, or what is termed its "retardation." It may also be stated that, with any given line, this time is dependent both upon the sensitiveness and the resistance of the indicating-instrument; by decreasing the sensitiveness or by increasing the resistance interposed by the instrument, the observed time is increased. Professor Wheatstone, in his celebrated experiment with the rotating mirror, measured the time which the discharged contents of his Leyden jar required to gather tension enough to spring over one tenth of an inch of air after it had passed through 440 yards of copper wire. Had he increased the length either of the spark or of the copper wire, he would necessarily have increased the observed time also. To-day any attempt to measure the velocity of an electric wave by frictional electricity would scarcely be expected to afford

\* Communicated by the Author.

exact results, the sensitiveness of such a means of indication being more than a thousand times less than that of even a Morse magnet. But this beautiful experiment was made before Morse magnets were invented, and fifteen years before a submarine cable was attempted, from which date exacter knowledge has been acquired of the principles involved; and much more delicate methods have consequently been devised. In all these methods, however, the same rule holds good: some work has to be performed by the current, either in moving a magnet or in neutralizing some given amount of opposite electricity; and it is only when this performance is completed that the indication is received.

It is obviously impossible to divide the length of wire operated upon by the time required to indicate a signal, and to call the quotient the "velocity of electricity" or even "the velocity of a wave." Nor would any statement of time under such circumstances be complete without all the data being given of resistance and inductive capacity of the line, and of resistance and of some relative constant of sensitivity of the indicating instrument. In all the older experiments these data were omitted, and we can only make an approximate estimate of them in most cases. When we do so and reduce the results to some imaginary line of unit resistance and unit inductive capacity, we find the reduced times agree for those with similar indicating systems. Where this was not the case, the reduced times are found to correspond with the relative degrees of sensitiveness of the indicating systems employed\*.

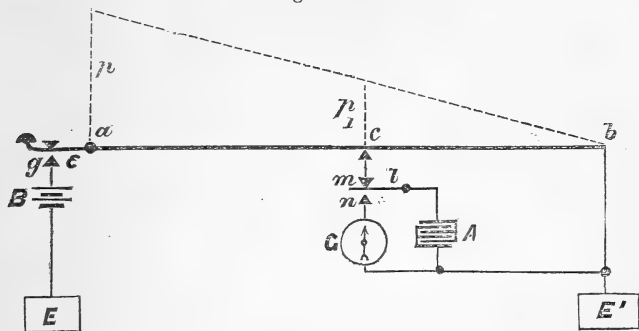
I have lately devised the following method, by which the electrical state of any given point in a line may be examined quantitatively at intervals of  $\frac{1}{1000}$  of a second or less after starting the electric impulse, so that the exact contour of a wave, from the first instant of its gaining sufficient strength to move a delicate mirror-galvanometer, may be measured from point to point to its maximum, and, if required, to its gradual disappearance. And by this method it becomes possible also to ascertain the speed of a wave, by observing the time which is taken for any given part of it to pass between two given points in the line.

This method is in principle as follows:—The conductor or line, *ab* (fig. 1), is furnished at one end (*a*) with a contact-key, *ε*, which can be pressed upon the contact-stud *g*, and puts in circuit the battery *B*, one pole of which is attached to an earth-plate *E*; at the other end (*b*) the conductor is put to a second earth-plate (*E'*). Some intermediate point, *c*, is

\* A tabulated statement of the reduced times from all the older measurements may be found in the 'Telegraph Journal' (1873), vol. i. p. 186.

put into connexion with a contact-stud  $m$ , against which the key ( $l$ ) presses. To  $l$  is connected one side of an accumulator,

Fig. 1.

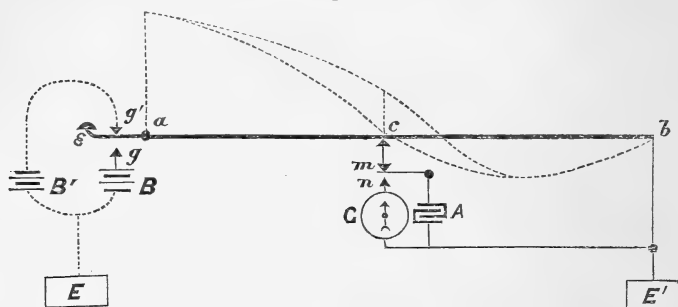


A, the other side of which is put to earth ( $E'$ ). A second contact-stud,  $n$ , connected through a galvanometer to the earth side of the accumulator, allows the latter to be discharged and the potential of its contents ascertained at any moment. The potential of the accumulator (whose capacity must be small in comparison with the capacity of the line), whilst in contact with the point  $c$ , may be assumed to be identical with that of  $c$ ; so that by depressing the key  $l$  the excursion of the galvanometer-needle indicates the electrical state which  $c$  had at the instant that  $l$  was separated from  $m$ . Now suppose an appreciable time to be required for any recognizable strength of wave to travel from  $a$  to  $c$ ; if the key  $\epsilon$  be depressed and make contact with  $g$  at the instant  $l$  is separated from  $m$ , it is evident that (no alteration having taken place in the potential at  $c$ ) no movement of the galvanometer will be observed. If, however, the key  $\epsilon$  be held down some time before  $l$  is separated from  $m$ , electricity will flow through, and each point along the resistance  $ab$  will assume the potential due to its position, from  $p$  (at  $a$ ) to zero (at  $b$ ), as indicated by the diagonal. The discharge of the accumulator will then give an excursion of the needle proportional to the potential ( $p_1$ ) of the point  $c$ . After contact of the battery at  $a$ , every point in the conductor takes a certain time before it attains the maximum potential due to its position. By gradually increasing the time which elapses between the contact of  $\epsilon$  with  $g$  and the break between  $l$  and  $m$ , the potential which the point  $c$  from interval to interval attains being observed, the contour of the wave is found. The point  $c$  may of course be chosen in any position between  $a$  and  $b$ , to suit the galvanometer or otherwise.

By a slight modification, the time which the neutral point

between two waves of opposite sign takes to arrive at  $c$  may be determined, and at the same time the contour of the vanishing and increasing waves. For this purpose it is necessary to furnish the key ( $\epsilon$ ) at the end ( $a$ ) of the conductor with a second contact-stud,  $g'$  (fig. 2), in connexion with a battery,  $B'$ , which

Fig. 2.



supplies opposite electricity to that supplied by  $B$ . The current of this battery ( $B'$ ) can either be kept flowing through until the wave from  $B$  is transmitted, or it may be inserted for a short interval only, as in working a line with reversed currents.

When with this arrangement  $\epsilon$  is depressed, the current of  $B$  flows into the conductor, and as it passes onwards neutralizes the electricity left in it by  $B'$  and recharges the line in the other direction. If the accumulator be discharged before the neutral point has reached  $c$ , the galvanometer shows electricity left by  $B'$ . If the neutral point has passed beyond  $c$ , it shows the opposite electricity—that of  $B$ . The neutral point having arrived at  $c$ , at the instant of separating  $l$  from  $m$ , no movement of the galvanometer is observed. The dotted curves in fig. 2 give a rough idea of the contour of waves produced in this manner.

To obtain results with this method, it is of course necessary to be provided with an apparatus which can be exactly adjusted to open the contact between  $l$  and  $m$  at any desired small interval of time after it has closed the contact between  $\epsilon$  and  $g$ .

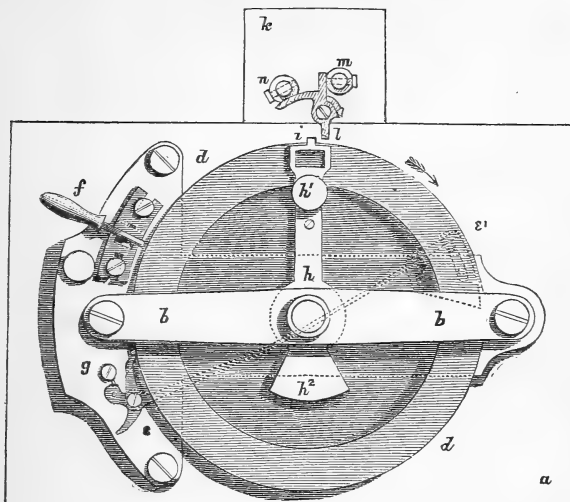
The apparatus which I use for this purpose, and, I believe, the only one which is applicable for the purpose, is the same that I had from Sir C. Wheatstone, and used for controlling the time-intervals given by means of an accumulator discharging through a known resistance, as described in the May Number of the *Philosophical Magazine*.

This apparatus, with the modifications which have been recently made in it, appears likely to be extremely useful for a variety of physical inquiries involving short definite intervals of



time; I will therefore give here a brief description of its mechanical construction. On the base *a* is (supported by pillars) a bridge, *b b*, in the middle of which is pivotted a brass disk with

Fig. 3.



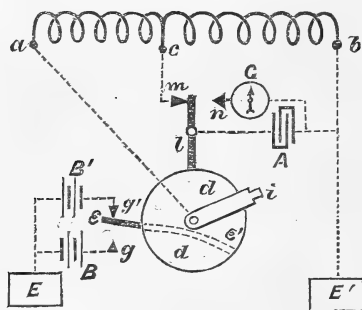
heavy rim (*dd*). At *e'* a band-spring is fixed which reaches to *e*, and at that end carries a catch centred on a pin. A small screw-head in the rim of the disk engages with this catch, and by means of it the spring may be forced backwards until a second protruding screw-head reaches the trigger *f*, by which it is locked. On releasing the trigger, the spring (recovering its position of rest) accelerates the disk until it is arrested against the contact-stud *g*, at which point it stops, whilst the disk is free to continue its revolution. The tension of the spring and position of the trigger are adjustable in order to "time" the period of revolution. On the top of the disk and turning with it is an arm, *h*, whose position is fixed by a clamp, *h'*. This arm is furnished with a protruding finger, *i*. In front of the board which carries the disk is a second board, *k*, which carries a small contact-key, *l*, working between contact-studs *m* and *n*, and retained indifferently against either of them by means of a spring at the side. The periphery of the disk is divided into 500 equal parts, the zero-point being in such a position that, the nonius on the arm *h* being placed coincident with it, the finger *i* just touches the insulated face of the key *l* (at rest on *m*) when the end of the spring *e* touches the contact-stud *g*. The arm *h* being placed in any other position and the disk released, the interval of time which elapses between the closing of contact

of spring  $\epsilon$  with stud  $g$  and the breaking of contact between  $l$  and  $m$  is shown by the engraved division upon which the index of the arm  $h$  stands. The disk, making two revolutions per second, each of the 500 divisions necessarily represents  $\frac{1}{1000}$  of a second, which is further divisible by the *nonius* to  $\frac{1}{10000}$ . It is of course assumed that the rate of rotation during a revolution is uniform, the retardation through friction being too minute to be appreciable in so short a time.

The original way of "timing" this apparatus was to start it and note the figures read in succession under the fibre of a small telescope when the disk was illuminated by half-second flashes of an induction-coil. The force of the spring and the position of the trigger were adjusted until the right velocity was obtained. Recently I have employed an equally accurate and easier way of "timing," by discharging an accumulator through a known resistance for some interval indicated by the disk, and calculating, from the initial and final readings of the galvanometer, the time according to the leakage-formula. If the two do not agree, I alter the spring until they do. The spring, however, is very constant; and no adjustment has been found to be necessary, except after taking the apparatus to pieces for alterations.

The manner in which the apparatus is connected up is shown in plan in fig. 4. The body of the disk  $dd$  is connected with

Fig. 4.



end  $a$  of the conductor ( $acb$ ). A contact  $g'$ , against which the spring  $\epsilon$  presses, puts the battery  $B'$  to the line when the disk is triggered; and another contact,  $g$ , puts in the opposite battery  $B$ , when it is released. This arrangement is used when the neutral point of two opposite waves is to be found; but when the contour of a single wave is required, the battery  $B'$  is omitted and  $g'$  connected to earth in order to facilitate the discharge of the line. The electricity of the point  $c$  goes through  $m$  and  $l$  to  $A$ , which it charges. When the disk rotates, the

arm  $i$  strikes the key  $l$  and throws it from the stud  $m$  (which is in connexion with the intermediate point  $c$ ) on to the stud  $n$ , which is connected with the galvanometer  $G$  and accumulator  $A$ . The end  $b$  of the conductor is to earth  $E'$ .

Through the kindness of Mr. Willoughby Smith, I have been enabled to make some measurements with this apparatus upon lengths of gutta-percha cable, lately manufactured by the Telegraph Construction and Maintenance Company, Limited, for submersion in the Red Sea. Measurements of this kind upon coiled cable containing iron in the covering are of less value than upon submerged cable; but as the amount of iron in the covering of this cable is comparatively small, I do not think that the magneto-electric currents set up were of great strength\*. I hope to be allowed the opportunity of making similar measurements upon a submerged and duplicated cable. In the mean time I give the results, by way of illustrating the employment of the method rather than professedly for their intrinsic value.

*Exp. 1.*—The length  $ac$  (fig. 1) consisted of 308 knots of coiled gutta-percha cable;  $bc$  of 3000 ohms wire-resistance, the end of which was to earth. The battery  $B$  was ten Minotto cells. Excursions of the needle of a mirror-galvanometer were read as follows, the index-hand ( $i$ ) of the rotating disk being advanced ten or more divisions between each observation.

Interval of time by disk.	Excursion of galvanometer- needle.
0.001 second . . .	0 division.
0.01     "     . . .	1     "
0.02     "     . . .	2 divisions.
0.03     "     . . .	4     "
0.04     "     . . .	7     "
0.05     "     . . .	13    "
0.06     "     . . .	18    "
0.07     "     . . .	24    "
0.08     "     . . .	30    "
0.09     "     . . .	34    "
0.10     "     . . .	39    "
0.15     "     . . .	57    "
0.20     "     . . .	68    "

\* Mr. Culley, experimenting on a circuit from Lowestoft to Holland and back, found "no trace of induction from the neighbouring wires through which the ordinary traffic was proceeding" (*Journ. Soc. Teleg. Eng.* 1875, vol. iv. p. 64). This fact indicates that the effect of coiling observed in some cables is not due to voltaic induction, but to magneto-induction, and is to be ascribed to the mass of iron of the covering, which, when the cable is coiled, acts like an electromagnet. Therefore a cable with very little iron in its covering should not behave very differently whether coiled or straight, whereas in a cable without iron the coiling should make no difference.

Table (continued).

Interval of time by disk.	Excursion of galvanometer- needle.
0.25 second . . .	75 divisions.
0.30     "     . . .	80     "
0.35     "     . . .	83     "
0.40     "     . . .	84     "
0.45     "     . . .	85     "

The potential of the battery was represented by 208 divisions; the potential of position of point *c*, or the maximum excursion (after 1 minute) was 99 divisions. Therefore after  $\frac{1}{100}$  of a second the point *c* had attained 1 per cent. of the potential due to its position, and after  $\frac{1}{4}$  second 75 per cent.

By plotting out these values, taking the time and the excursions as coordinates of a curve, the contour of the wave is obtained.

*Exp. 2.*—308 knots of coiled cable between *a* and *c*, *b c* being a resistance-coil of 3000 ohms. Two batteries (connected as in fig. 2), B and B', each ten Minotto cells.

Interval by disk.	Excursion of galvanometer- needle.
0.01 second . . .	—50 divisions.
0.05     "     . . .	—29     "
0.10     "     . . .	+10     "
0.15     "     . . .	39     "
0.20     "     . . .	57     "
0.25     "     . . .	67     "
0.30     "     . . .	73     "
0.35     "     . . .	78     "
0.40     "     . . .	80     "
0.45     "     . . .	82     "

Plotted out, this curve shows that the neutral point passed *c* 0.085 second after reversal of the battery at *a*.

*Exp. 3.*—Between *a* and *c*, a coiled cable 114 knots; between *b* and *c*, a resistance-coil of 1000 ohms. Two batteries as before.

Interval by disk.	Excursion of galvanometer- needle.
0.005 second . . .	—146 divisions.
0.006     "     . . .	—106     "
0.007     "     . . .	—70     "
0.008     "     . . .	—38     "
0.009     "     . . .	0     "
0.010     "     . . .	+47     "
0.011     "     . . .	85     "
0.012     "     . . .	117     "
0.013     "     . . .	168     "

*Exp. 4.*—The same arrangements as in *exp. 3*, with the exception of the resistance (*b c*), which was reduced to 500 ohms.

Interval by disk.	Excursion of galvanometer-needle.
0·003 second . . .	—39 divisions.
0·004     " . . .	—21     "
0·005     " . . .	— 2     "
0·006     " . . .	+13     "

The reduction of the end resistance (*b c*) to half its value, therefore, altered the time of the neutral point passing from 0·009 to 0·005 second.

*Exp. 5.*—The resistance was increased to 5000 ohms, the other details being the same as in the preceding experiment.

Interval by disk.	Excursion of galvanometer-needle.
0·015 second . . .	—182 divisions.
0·016     " . . .	—122     "
0·017     " . . .	— 61     "
0·018     " . . .	+ 2     "
0·019     " . . .	+ 78     "
0·020     " . . .	+106     "
0·021     " . . .	+184     "

This shows an increase of the time by an increase of the end resistance. If these end resistances represent the resistances offered by the coils of receiving-instruments, the results of the last three experiments confirm our experience that by increasing the instrument-resistance without correspondingly increasing the sensitiveness, we lessen the rate of signalling.

Fig. 5.



By means of two lengths (114 and 190 knots) of coiled cable, I was enabled, in a total length of 304 knots, to put the accumulator in contact with two intermediate points, viz. *c* (at 114 knots from *a*) and *c'* (at 190 knots from *a*).

*Exp. 6.*—Accumulator inserted at *c* (fig. 5) 114 knots from *a*; and *b* directly to earth. Two batteries of five cells each.

Interval by disk.	Excursion of galvanometer-needle.
0·032 second . . .	—24 divisions.
0·033     " . . .	—17     "
0·034     " . . .	—12     "
0·035     " . . .	— 7     "
0·036     " . . .	— 2     "
0·037     " . . .	+ 3     "
0·038     " . . .	7     "
0·039     " . . .	12     "
0·040     " . . .	20     "

*Exp. 7.*—Accumulator inserted at  $c'$  190 knots from end  $a$ .

Interval by disk.	Excursion of galvanometer-needle.
0·045 second . . .	—12 divisions.
0·046   "   . . .	—10   "
0·047   "   . . .	—7   "
0·048   "   . . .	—4   "
0·049   "   . . .	0   "
0·050   "   . . .	+5   "
0·051   "   . . .	7   "
0·052   "   . . .	9   "
0·053   "   . . .	11   "

*Exp. 8.*—Accumulator inserted at end of cable, 304 knots from  $a$ , and a resistance of 100 ohms between  $b$  and earth.

Interval by disk.	Excursion of galvanometer-needle.
0·04 second . . .	—7 divisions.
0·05   "   . . .	—4   "
0·06   "   . . .	—2   "
0·067   "   . . .	0   "
0·07   "   . . .	+1   "
0·08   "   . . .	2   "
0·09   "   . . .	5   "
0·10   "   . . .	7   "

Whatever may be the precise effect of the cable being coiled, upon the results, it is probably greater at the first moment than afterwards. From this cause the time of the neutral point crossing may be in *exp. 6* higher than it would have been had the cable been submerged, less so in *exp. 7*, and least so in *exp. 8*. As it was, however, the times were as follows when the neutral point passed  $c$ ,  $c'$ , and  $b$  :—

Point passed.	Time.
$c$ (114 knots from $a$ ) . . . .	0·036 second.
$c'$ (190   "   ) . . . .	0·049   "
$b$ (304   "   ) . . . .	0·067   "

Between  $c$  and  $c'$  the wave passed through 76 knots in 0·013 second, and between  $c'$  and  $b$  114 knots in 0·018 second. In this sense we can say that in this identical 304 knots of coiled cable, the wave in question travelled at the rate of about 6000 knots per second.

In carrying out these experiments, I observed that the same series repeated gave the same results.

The accumulator employed was one made of mica plates with tinfoil coatings and cemented with shellac. Its capacity was 0·33 microfarad, its resistance at the time of making the ex-

periments about 300,000 megohms. The capacity of the accumulator introduces an error, because it requires to be supplied with as much electricity as a mile of submarine cable, or as 50 to 100 miles of land line. The effect which it has on the form of the wave is therefore the effect of an error of position of less than 1 mile in the cable. Now in a length of 200 or 300 miles this effect is of very little practical importance and may be neglected; but if we have to measure contours of curves or speeds of waves in short cables or land lines, the accumulator employed should have a proportionally small capacity.

When two batteries are used (that is, the waves are observed by reversed currents), the second battery should be introduced at the same instant that the first battery-circuit is interrupted, not to allow time for the charged state of the cable to fall. This was first done with two batteries of ten cells and five cells respectively connected in opposite directions, so that when the current passed through them all, the resultant electromotive force was  $10 - 5 = 5$  cells in one direction. The reversal was made by simply shortcircuiting the ten cells and allowing the five others to operate alone in the reverse direction. This plan was not without objection; and an arrangement was therefore made by which the interval between the removal of one battery and the introduction of the other was reduced to a minimum by means of a spring following the contact, and arrested just before the reversal, by which the interval was made less than 0.0005 second.

The time-contact apparatus may be used so as approximately to imitate any two successive currents given in the ordinary ways of telegraphing; and the speeds and contours of the waves under such conditions can then be ascertained. In these experiments, as in actual telegraphing, we can leave out of consideration the absorption of electricity into the dielectric material. Practically we have to deal only with what may be termed the "free charge" of the cable, or that which is free to be quickly discharged. The electricity which takes time to creep into and out of the material does not influence perceptibly the rate of signalling or the contour of waves.

In conclusion, I must thank Mr. Willoughby Smith for affording me opportunities of practically illustrating this method, and Mr. May for his kindness and assistance in the matter.

25 Cumberland Terrace, Regent's Park,  
September 12, 1876.

XLII. *Attempt at a Theory of the (Anomalous) Dispersion of Light in Singly and Doubly Refracting Media.* By Professor E. KETTELER\*.

**A**FTER establishing, as I believe, by my own experiments, especially on gases, as well as making critical use of all extant observations, the empirical side of the theory of dispersion as a whole, I have further been able also to explain its connexion with the elliptical polarization of reflection and refraction†. The dispersion-formula constructed by me includes in itself the very phenomenon, afterwards discovered, of anomalous dispersion; and it satisfies experience not only for the more feebly absorbed transmitted rays, but also, apparently, for the perfectly dark regions of the spectrum, which are accessible only by the elliptical polarization of the reflected light.

Anomalous dispersion has meanwhile effected a complete revolution in reference to theory; for it has become evident that in dioptric processes the æther alone will not suffice; and so the view represented chiefly by Boussinesq, Sellmeier, and myself, that the æthereal and corporeal particles vibrate together, is gradually more and more making way. And then a really tenable theory of dispersion, based on this conception, promises, together with a more profound understanding of light-motion in general, some disclosures concerning the structure and constitution of the aggregates, composed of æthereal and corporeal matter, which form the only ponderable media.

The most recent treatise on anomalous dispersion we owe to Helmholtz‡. He also starts from a convibration of ponderable particles; and by assuming only the simplest possible mechanism in reference to the reciprocal action between them and the æther particles, he readily arrives at the construction of the two requisite differential equations. Helmholtz rejects the assumption, made by the older physicists, that the æthereal and corporeal particles attract or repel each other point for point, and substitutes for it an action like that of a vibrating pendulum on the surrounding air. Lastly, he shows emphatically that, on account of the gradual conversion of light into heat (that is, absorption of the regular oscillatory motion), co-

\* Translated from a separate impression, communicated by the Author, from the *Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westphalens*, Jahrg. xxxiii., 4th series, vol. ii.

† "Das Complexe als Ausdruck des Zusammenhangs zwischen der elliptischen Polarisation der Spiegelung und Brechung und der Dispersion der Farben," *Verh. des naturhist. Vereins für Rheinland-Westphalen*, 1875.

‡ *Monatsber. der Berl. Akad.* Oct. 1874; *Pogg. Ann.* vol. cliv. p. 582.



efficients of direct friction must be introduced into the calculus. In fact the success of this speculation depends absolutely on the presence of the terms conditioned by friction ; and if these be eliminated by putting the friction-coefficient  $=0$ , the dispersion-curve obtained loses all resemblance to that required by experiment.

Helmholtz's treatise, however, appears to me an explanation *ad hoc*, inasmuch as it neglects the two different kinds of elliptic polarization established by experiment (of glass and metals, the positive and the negative), and also the modification effected in the velocity of propagation by the translation of a medium, or gives no account of them, and, lastly, as it contradicts the theories of reflection which have been hitherto held by Fresnel, Cauchy, and myself, all of which require, at least for the boundary surface, a simple relation between the *vires vivæ* and the ratio of refraction.

To me it becomes difficult, especially after the results of my last work, to abandon the representation that a refraction-index variable with the angle of incidence belongs to the metals, and that absorption plays, instead of a primary, only a secondary part. After much pains I have at last been compelled to decide that it is impossible to arrive at expressions serviceable in every direction, even by extending Helmholtz's assumptions by the addition of new terms ; while these hypotheses appear to me in part not unobjectionable, even theoretically.

For experiment shows the course of the dispersion-curve to be independent of the condition of the aggregate. If now we consider that only towards very rapid motions does the æther behave as a solid body, while its resistance to slow displacements, even those of the light particles of gas flying about in space, entirely vanishes, and that, in agreement with this, according to observations of aberration, the translation of a medium which takes place perpendicular to the direction of the rays leaves the velocity of propagation of them unaffected, the mechanism of the reciprocal action between the æthereal and corporeal particles may be in reality more complicated than Helmholtz assumes it to be. According to our simple consideration the influence of the corporeal particles upon large oscillation-periods becomes just  $=0$ , and hence, for all aggregate forms, the corresponding velocity of propagation  $=1$ , while Helmholtz's theory gives this velocity, at least for gases, as 0.

If now I proceed to set up a theory of my own, in my opinion the difficulties indicated may be avoided in the following simple way :—

2. We confine ourselves in the following to media at rest,

and treat first of one whose ponderable molecules are arranged isotropically and are optically simple in their chemical quality, so that its dispersion-curve exhibits only a single absorption-streak.

This presupposed, let  $m, m'$  be the masses contained in the unit of volume of the æthereal and corporeal particles, and  $\rho, \rho'$  the relative excursions. Then  $m \frac{d^2 \rho}{dt^2}, m' \frac{d^2 \rho'}{dt^2}$  are the forces acting upon them, measured by the acceleration. On the other hand these forces will be composed in the following manner.

If  $e$  is the constant of the elastic deformation of the pure æther, so that, for this,

$$m \frac{d^2 \rho}{dt^2} = e \frac{d^2 \rho}{dx^2},$$

where  $x$  is to be referred to the direction of propagation, then, for the interior of a ponderable medium, to the force  $e \frac{d^2 \rho}{dx^2}$  another is added, arising from the reciprocal action of the corporeal particles. This, on account of the infinitely less mass and minuteness of the æther particles, as well as their facility of displacement, will likewise be a deformation-force  $E \frac{d^2 \rho}{dx^2}$ \*, of which the characteristic (at present unknown)  $E$  is therefore added as increment to  $e$ . We may perhaps suppose that by the resistance of the corporeal particles a like effect is produced as if the tension of the æther were altered. We have consequently, for the motion of the æther particles in the interior,

$$m \frac{d^2 \rho}{dt^2} = (e + E) \frac{d^2 \rho}{dx^2}. \quad . \quad . \quad . \quad . \quad (1\Delta)$$

On the other hand, with respect to the vibrations of the more discretely distributed corporeal particles, the assumption is *à priori* admissible, that their amplitudes are much less than those of the æther particles. We further consider the presence of the former solely as a hindrance to the free motion of the æther. Now the force acting on the corporeal particles may either depend on the curvature of the wave-line uniting them†, or on

\* At least, as the sequel will show, it is equivalent to such a force.

† If the corporeal particles of a medium effected no resistance at all to the motion, the equation for it would be the same as if the masses  $m$  and  $m'$  were firmly united to one another—that is,

$$(m + m') \frac{d^2 \rho}{dt^2} = e \frac{d^2 \rho}{dx^2}.$$

Even this extreme case the theory will have to take into account.

their distance from the position of equilibrium. We will assume as more probable that it depends on both. Accordingly this force also will certainly in part present itself as a deformation-force  $E' \frac{d^2 \rho'}{dx^2}$ , but of course as a deformation-force of the corporeal particles displaced by the vibrating æther. To this will then be added a directly acting displacing force, which, as it is proportional to the existing excursion, may be denoted by  $K\rho'$ , and of which it remains uncertain whether it proceeds immediately from the pressure of the æther particles or is only indirectly produced by the reaction of the corporeal particles. It is self-evident that  $e$  is positive, while the sign of the rest of the forces is at first undetermined. And since the course of the dispersion is, as we have intimated, independent of the state of the aggregate, the contents of  $E'$  and  $K$  do not refer to those forces which in solid bodies hold together their constituent parts, but much rather, analogous to the behaviour of vapours endowed with rotation-polarization, to the forces between the constituents of the molecules themselves, or, in fine, to the reciprocal forces between the æthereal and corporeal particles, inasmuch as they depend on the quality and form of the latter. We should therefore have, for the motion of the corporeal particles,

$$m' \frac{d^2 \rho'}{dt^2} = E' \frac{d^2 \rho'}{dx^2} + K\rho'. \quad . \quad . \quad . \quad (1.B)$$

The only remaining difficulty consists in the treatment of the functions  $E$ ,  $E'$ ,  $K$ , which, as is acknowledged, must contain implicitly the vibration-period or wave-length. Here the plausible assumption presents itself, that the three preceding forces, conditioned by the presence of ponderable particles, and flowing from their reciprocal action in equal degree with the æther, are rigorously proportional to one another. Correspondingly we will put

$$E = \alpha\epsilon, \quad E' = \alpha\epsilon', \quad K = \alpha\kappa, \quad . \quad . \quad (2)$$

where  $\epsilon$ ,  $\epsilon'$ ,  $\kappa$  denote constants which are given with the quality of the molecular constitution (or the density), and  $\alpha$  alone depends on the motion (consequently on the dynamic condition) of the medium. Hence comes, definitively :—

$$m \frac{d^2 \rho}{dt^2} = (e + \alpha\epsilon) \frac{d^2 \rho}{dx^2};$$

$$m' \frac{d^2 \rho'}{dt^2} = \alpha\epsilon' \frac{d^2 \rho'}{dx^2} + \alpha\kappa\rho'.$$

To integrate these equations we put :—

$$\left. \begin{aligned} \rho &= A \cos 2\pi \left( \frac{t}{T} + \frac{x}{l} - \Theta \right); \\ \rho' &= A' \cos 2\pi \left( \frac{t}{T} + \frac{x}{l} - \Theta \right). \end{aligned} \right\} \quad . \quad . \quad . \quad (3)$$

Introducing these values, the equations are transformed into the following :—

$$\begin{aligned} \frac{m}{T^2} &= \frac{e + \alpha \epsilon}{l^2}, \\ \frac{m'}{T^2} &= \alpha \frac{\epsilon'}{l^2} - \alpha \kappa', \end{aligned}$$

where  $\kappa' \left( = \frac{\kappa}{4\pi^2} \right)$  is put for brevity. These conditions must, then, be satisfied between the constants of the expressions (3), if they are to be admissible as integrals of equations (1). Eliminating  $\alpha$  from them, we get, first,

$$\frac{m}{T^2} = \left( e - \frac{\frac{m'}{T^2} \epsilon}{\kappa' - \frac{\epsilon'}{l^2}} \right) \frac{l}{l^2},$$

and, taking into account the relation  $l = \omega T$ ,

$$\omega^2 \left( m + \frac{m' \epsilon}{\kappa' l^2 - \epsilon'} \right) = e.$$

Lastly, we introduce the velocity of propagation  $v$  valid for the universal æther ( $m' = 0$ ), for which  $v^2 = \frac{e}{m}$ , and, for abbreviation, put

$$\frac{\epsilon'}{\kappa'} = L^2, \quad \frac{m' \epsilon}{m \epsilon'} = D.$$

Then the refraction-ratio  $n$  receives the finally valid form

$$n^2 - 1 = \frac{D}{\frac{l^2}{L^2} - 1}, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which corresponds to the above requirement, viz. that for an infinitely great wave-length  $l = T = \infty$  the refraction-ratio  $n$  become  $= 1$ ; while for an infinitely little wave-length we obtain the limiting value  $n'^2 = 1 - D$ , against which, even inasmuch as when  $D$  is positive it becomes less than 1, theoretic objections are hardly admissible.

For the variable  $\alpha$  we find

$$\alpha = -\frac{m'\omega^2}{\kappa' l^2 - \epsilon'} = -\frac{\frac{m'}{\epsilon'} \omega^2}{\frac{l^2}{L^2} - 1}.$$

3. In order to proceed from the moving forces to the *vires vivæ*, let us imagine the particles of the medium brought out of their position of equilibrium into a relative position which would correspond as extreme to any internal wave-length  $l$  for any instant, and kept in this position by a suitable force. The tension thus produced is the same as if no ponderable particles were present. If the medium is then left to itself, the æther particles press back to the position of equilibrium, carry with them the corporeal particles, and the previous tension-force is converted into *vis viva*, which distributes itself to both kinds of molecules. Both pass the position of equilibrium with an energy which may be denoted respectively by  $mC^2$  and  $m'C'^2$ , or by  $m \frac{A^2}{T^2}$ ,  $m' \frac{A'^2}{T'^2}$ . And since in the æther of space with identical displacement (*i. e.* identical  $A$  and  $l$ ) an equal tension-force is developed, which now generates the maximum *vis viva*  $mC_0^2 = m \frac{A^2}{T_0^2}$ , we have

$$m \frac{A^2}{T^2} + m' \frac{A'^2}{T'^2} = m \frac{A^2}{T_0^2},$$

which relation, on account of  $l = vT_0 = \omega T$  and  $v^2 = \frac{e}{m}$ , changes into

$$m \frac{A^2}{T^2} + m' \frac{A'^2}{T'^2} = \frac{e}{l^2} A^2, \quad . \quad . \quad . \quad (5A)$$

or even, if instead of the maximal the variable oscillation-velocities or excursions be at the same time introduced, into

$$n^2 - 1 = \frac{m' \rho'^2}{m \rho^2}. \quad . \quad . \quad . \quad . \quad . \quad (5B)$$

The so-called refracting force is therefore equal to the ratio in which a given *vis viva* is distributed to corporeal and æthereal particles.

This presupposed, we have to examine whether, and under what conditions, our dispersion-equations are compatible with the theorem here deduced. To this end we multiply the two differential equations respectively with  $\rho$ ,  $\rho'$ , and add. We thus get

$$m\rho \frac{d^2\rho}{dt^2} + m'\rho' \frac{d^2\rho'}{dt^2} = \epsilon\rho \frac{d^2\rho}{dx^2} + \alpha\epsilon\rho \frac{d^2\rho}{dx^2} + \alpha\epsilon'\rho' \frac{d^2\rho'}{dx^2} + \alpha\kappa\rho'^2,$$

and, after integrating,

$$\frac{m}{T^3} \rho^2 + \frac{m'}{T^2} \rho'^2 = \frac{e}{l^2} \rho^2 + \alpha \left( \frac{\epsilon}{l^2} \rho^2 + \frac{\epsilon'}{l^2} \rho'^2 - \kappa' \rho'^2 \right).$$

Comparison with equation (5A) shows immediately that the condition to be fulfilled is the following :—

$$\frac{\alpha}{l^2} \rho^2 \left[ \epsilon + (\epsilon' - \kappa' l^2) \frac{\rho'^2}{\rho^2} \right] = 0.$$

If we omit the first factor and substitute for  $\frac{\rho'}{\rho}$  its value from (5B), we obtain

$$\epsilon - \epsilon' \left( \frac{l^2}{L^2} - 1 \right) \frac{\rho'^2}{\rho^2} = 0,$$

and also

$$n^2 - 1 = \frac{D}{\frac{l^2}{L^2} - 1}.$$

The condition sought is therefore no other than the dispersion-formula itself; and accordingly *it forms the requisite complement to the theorem of the refracting force as the ratio of the vires vivæ of the corporeal and æthereal particles.*

4. Media which would in strictness satisfy for the entire compass of the radiation the simple formula of dispersion hitherto developed have not yet been observed. From this it will be inferred that by far the preponderating number of substances possess, instead of one, several zones with a complex refraction-ratio (absorption-streaks). In regard to the treatment of these media, the vibrating corporeal masses will have to be analyzed into as many optico-chemical elements as there are absorption-streaks present. If the number were  $n$ , we should get  $n$  differential equations for the vibrations of the  $n$  different corporeal qualities of the masses  $m'$ , and, besides, for the vibrations of the æther the deformation-equation of the æther of space increased by  $n$  additional terms. We should thus obtain :—

$$\left. \begin{aligned} m \frac{d^2 \rho}{dt^2} &= (e + \alpha_1 \epsilon_1 + \alpha_2 \epsilon_2 + \dots) \frac{d^2 \rho}{dx^2}; \\ m'_1 \frac{d^2 \rho'_1}{dt^2} &= \alpha_1 \epsilon'_1 \frac{d^2 \rho'_1}{dx^2} + \alpha_1 \kappa_1 \rho'_1; \\ m'_2 \frac{d^2 \rho'_2}{dt^2} &= \alpha_2 \epsilon'_2 \frac{d^2 \rho'_2}{dx^2} + \alpha_2 \kappa_2 \rho'_2 \dots \end{aligned} \right\} \dots \quad (6)$$

Integration and the elimination of  $\alpha$  would now give

$$\omega^2 \left( m + \frac{m'_1 \epsilon_1}{\kappa'_1 l^2 - \epsilon'_1} + \frac{m'_2 \epsilon_2}{\kappa'_2 l^2 - \epsilon'_2} + \dots \right) = e.$$

And if again, generally, we put

$$\frac{\epsilon'}{\kappa'} = L^2, \quad \frac{m'\epsilon}{m\epsilon'} = D,$$

then comes definitively

$$n^2 - 1 = \Sigma \frac{D}{l^2 - L^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Now, to compare this expression with experience, or to bring it into a more convenient form for numerical treatment, let it be considered that the entire spectrum accessible to accurate measurement comprises only about two octaves, and that it is affected by the absorptions of the least as well as of the greatest wave-lengths. Let us imagine, further, the  $l^2$  laid down as abscissæ, and the individual function-values of the same, namely the terms contained under the sign of summation, each by itself, as ordinates. We thus obtain  $n$  particular curves; and the total ordinate is the sum of all the particular ones. Each of these part-curves runs according to a hyperbolic law; their centre is at  $L$ ; to the right of this it rises higher, to the left sinks lower, than the horizontal asymptote; and its steepness diminishes rapidly on both sides. In consequence of this, all curves whose middle point is proportionally distant from the boundaries of the accessible spectrum exert only an approximately constant influence upon the total curve obtained between them. If the influence of the ultra-violet region preponderates, the curve is raised; but if the ultra-red region is the stronger, it is lowered. In both cases we should have a medium with only feeble dispersion, and the mean refraction-ratio of which would be either above or below 1, so that  $n_m^2 - 1 = a$ .

To these distant curves comes now the influence of one of those whose centre falls either just within the visible spectrum or at least lies sufficiently near its limits. The total curve then attains the form

$$n^2 - 1 = a + \frac{D'}{l^2 - L^2}.$$

And if for  $a$  its value above given be introduced, and therefore logically  $a$  be referred to the ordinates whose middle point is free from local elevation, and, lastly,  $L = l_m$ ,  $D' = Dn_m^2$  be put, we get

$$(n^2 - n_m^2)(l^2 - l_m^2) = Dn_m^2 l_m^2 \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and also

$$(\omega_m^2 - \omega^2)(l^2 - l_m^2) = D.$$

The law of dispersion expressed by these equations is the same as that which I have deduced from experiment, especially for sulphide of carbon, and to which, in my treatise on the "Complex," I have referred all the theoretical consequences. If in (8) we replace  $l$  by  $\frac{\lambda}{n}$ , understanding by  $\lambda$  the wave-length belonging thereto in the æther of space, and solve according to  $n$  the equation thus transformed, we get, for the form of the true curve of the dispersions  $n=f(\lambda)$ , the law of formation

$$n = \frac{1}{2}n_m \left( \sqrt{\left(1 + \frac{\lambda}{\lambda_m}\right)^2 - D} \pm \sqrt{\left(1 - \frac{\lambda}{\lambda_m}\right)^2 - D} \right); \quad (9A)$$

and for the associated internal wave-length  $l$ , on account of the symmetry of equation (8),

$$l = \frac{1}{2}l_m \left( \sqrt{\left(1 + \frac{\lambda}{\lambda_m}\right)^2 - D} \mp \sqrt{\left(1 - \frac{\lambda}{\lambda_m}\right)^2 - D} \right). \quad (9B)$$

If we notice, finally, that the signs are unequivocally\* determined by this, that the ordinates of the curve (9A), in distinction from the curve (9B), never become infinite, it shows indeed the form required by experiment. It is found to be  $\lambda > \lambda_m$  to the right of the middle line,  $n > n_m$  above the horizontal asymptote,  $\lambda < \lambda_m$  to the left, and  $n < n_m$  below. The curve has, further, between two determinate limiting wave-lengths  $\lambda'_0, \lambda''_0$  (the theoretic limits of the absorption-band), an apparently variable interruption. Namely, for this reason the refraction-ratios become complex ( $n = a \pm b\sqrt{-1}$ ), a proof that here the vibrations of the æthereal and corporeal particles no longer obey the usual laws. The dispersion-force  $D$ , external and internal limiting wave-lengths ( $\lambda'_0, l'_0; \lambda''_0, l''_0$ ), and limiting indices of refraction ( $n'_0, n''_0$ ) are connected with  $\lambda_m, l_m$  by the relation

$$D = \left(1 - \frac{n_0^2}{n_m^2}\right)^2 = \left(1 - \frac{l_0^2}{l_m^2}\right)^2 = \left(1 - \frac{\lambda_0}{\lambda_m}\right)^2. \quad (10)$$

According to experiment, for sulphide of carbon as for all those media to which the foregoing formula is applicable at least approximately,  $n_m^2$  is greater than 1; consequently the influence of the ultra-violet absorptions is the predominant one. For all these media the next most influential particular curve lies within the region of the shorter wave-lengths. Only the third rank, for the majority of them, does any action from the ultra-red end also make itself perceptible.

If, finally, for the  $n$ th particular curve  $D_n$  be taken as = 1

\* Herewith at once falls an objection raised by Helmholtz.



and  $L_n = \infty$ , equation (7) can also be written thus,

$$n^2 = \frac{D_1 L_1^2}{l^2 - L_1^2} + \frac{D_2 L_2^2}{l^2 - L_2^2} + \dots;$$

and this relation will, as an empirical one, equally well represent the connexion between index of refraction and wave-length provided only the number of terms be made dependent on experiment. As for the rest, the hyperbolic character of the particular curves permits  $n$  to be exchanged for its reciprocal value  $\omega$ ; and thus the formula

$$\omega^2 = \frac{A}{l^2 - B} + \frac{C}{l^2 - D} + \dots$$

may lay claim to equal admissibility; I have proved and confirmed it in the most comprehensive manner.

From the result of this trial I here conversely draw the conclusion that *just so the theoretical formula (7) accords perfectly with the best measurements hitherto made extending over an interval of two octaves.*

5. If we proceed to the discussion of the *vires vivæ* in this more general case also, it will suffice, in the first place, to point out that generalizing the method of reasoning carried out under § 3 leads at once to the corresponding relation

$$n^2 - 1 = \frac{\Sigma m' \rho'^2}{m \rho^2}.$$

Self-evidently, instead of the *vires vivæ* of the single molecules, we can bring into it the *vis viva* of the entire corporeal mass; we have then to put

$$\Sigma m' \rho'^2 = \rho'^2_m \Sigma m'.$$

In regard to the compatibility of the same with the above differential equation, by an analogous procedure we obtain

$$m \rho \frac{d^2 \rho}{dt^2} + \Sigma m' \rho' \frac{d^2 \rho'}{dt^2} = \epsilon \rho \frac{d^2 \rho}{dx^2} + \Sigma \alpha \left( \epsilon \rho \frac{d^2 \rho}{dx^2} + \epsilon' \rho' \frac{d^2 \rho'}{dx^2} + \kappa \rho'^2 \right), \quad (11 A)$$

and the condition-equation becomes

$$\Sigma \alpha \left( \epsilon \rho \frac{d^2 \rho}{dx^2} + \epsilon' \rho' \frac{d^2 \rho'}{dx^2} + \kappa \rho'^2 \right) = 0$$

or

$$\Sigma \frac{\alpha}{l^2} \rho^2 \left[ \epsilon + (\epsilon' - \kappa' l^2) \frac{\rho'^2}{\rho^2} \right] = 0.$$

Now, as the medium under consideration (a mixture of gases, suppose, or a solution of salts) is composed quite arbitrarily of several simple constituents, each of which is characterized by  $\epsilon$ ,  $\epsilon'$ ,  $\kappa'$ , and  $\alpha$ , and we do not assume that these constituents individually exert reciprocal action upon one another, the fore-

going total condition resolves itself into  $n$  particular conditions. Hence we shall have

$$\epsilon_1 + (\epsilon'_1 - \kappa'_1 l^2) \frac{\rho_1'^2}{\rho^2} = 0,$$

$$\epsilon_2 + (\epsilon'_2 - \kappa'_2 l^2) \frac{\rho_2'^2}{\rho^2} = 0, \dots$$

Bearing this in mind, then the total result of the investigation hitherto is comprehended in the following most abbreviated form :—

$$\frac{\rho'^2}{\rho^2} = \frac{\epsilon}{\kappa' l^2 - \epsilon'}, \quad n^2 - 1 = \frac{\Sigma m' \rho'^2}{m \rho^2} \dots \dots (11)$$

6. On account of the nature of the last two equations, it is of course impossible to bring them into the form  $n=f(\lambda)$  and therefrom deduce rigorously the properties of the true dispersion-curve. Indeed, for a general view, what was said above respecting the approximative formula (9) is sufficient. It is thence obvious that, if we go through the middle point of a particular curve  $n$  of equation (11), we at the same time pass through an apparent inconstancy in the true dispersion-curve (an absorption-streak). If on the two sides of this middle line the  $\rho_n'^2$  of equation (11) has opposite signs, so that the particular *vis viva*  $m'_n \rho_n'^2$  on the right is added to the other terms, but is subtracted from them on the left, within the limits of the absorption-streak the wave-length  $l$  and index of refraction  $n$  become complex. Now I have shown, in my memoir cited above\*, that in this case the reflected and refracted light is very elliptically polarized, and that in the transmitted wave, not merely do the æther particles suffer a sudden change of phase  $\chi$ , but likewise the corporeal particles undergo a *different* alteration of phase  $\chi'$ . If, now, the actual excursion of the particles of both kinds, within the limits of the complex zone, are denoted by  $\rho'_0, \rho_0$ , it follows, from a generalization of the point of view there developed, that the existing equation

$$(a + b\sqrt{-1})^2 - 1 = \Sigma \frac{m'(\mathbf{r}'_1 + \mathbf{r}'_2\sqrt{-1})^2}{m(\mathbf{r}_1 + \mathbf{r}_2\sqrt{-1})^2}$$

divides into the two following :—

$$\left. \begin{aligned} a^2 - b^2 - 1 &= \Sigma \frac{m' \rho_0'^2}{m \rho_0^2} \cos 2(\chi' - \chi); \\ 2ab &= \Sigma \frac{m' \rho_0'^2}{m \rho_0^2} \sin 2(\chi' - \chi). \end{aligned} \right\} \dots \dots (12)$$

\* *Verhandl.* [4] vol. ii. p. 93.

If, then, for the  $n$ th zone, and for the limit-point  $G'$  to the right of it, we have  $\chi' - \chi = 0$ , the whole of the particular excursions will, with the entry into the absorption-streak, simultaneously undergo such differences of phase; but these reach at the middle line only an insignificant maximum, and on the other side of it sink again to zero. Only for  $(\rho'_0, \rho_0)_n$  does  $2(\chi' - \chi)$  rise considerably; it reaches, at least with very feeble dispersion, at the middle line the value  $\pm 90^\circ$  (corresponding to  $\pm \frac{1}{4}l$ ), and for the limit-point  $G''$  on the left the value  $\pm 180^\circ$  (or  $\pm \frac{1}{2}l$ ); so that here, indeed, again  $b=0$ , and therewith the curve becomes real, but the sign of  $\rho_n^2$  in equation (11), in accordance with the hypothesis, changes into its opposite.

Forming, lastly, the resulting ratio of the *vires vivæ* for the interior of the complex zone, we can put

$$\frac{R'_0 \Sigma m'}{R_0^2 m} = \sqrt{(a^2 - b^2 - 1)^2 + 4a^2 b^2}.$$

All the calculations can now be carried out for a substance with only one inconstancy in the spectrum ( $n_m=1$ ), consequently with omission of the symbol of summation. We find, in the first place, for very small dispersive forces,

$$\left. \begin{aligned} \frac{m' \rho_0'^2}{m \rho_0^2} &= \sqrt{D} = \sqrt{\left(1 - \frac{\lambda_0}{\lambda_m}\right)^2} = \sqrt{(1 - n_0^2)^2}, \\ \sin 2(\chi' - \chi) &= \sqrt{\frac{\lambda - \lambda_0}{\lambda_m - \lambda_0}}, \quad \cos 2(\chi' - \chi) = \sqrt{\frac{\lambda_m - \lambda}{\lambda_m - \lambda_0}} \end{aligned} \right\} (13)$$

We then obtain, pursuant to the above convention:—

Left of the middle.	Right of the middle.
$a^2 - b^2 - 1 = - \frac{\lambda_m - \lambda''_0}{\lambda_m} \sqrt{\frac{\lambda_m - \lambda}{\lambda_m - \lambda''_0}},$	$+ \frac{\lambda'_0 - \lambda_m}{\lambda_m} \sqrt{\frac{\lambda - \lambda_m}{\lambda'_0 - \lambda_m}}$
$2ab = \pm \frac{\lambda_m - \lambda''_0}{\lambda_m} \sqrt{\frac{\lambda - \lambda''_0}{\lambda_m - \lambda''_0}},$	$\pm \frac{\lambda'_0 - \lambda_m}{\lambda_m} \sqrt{\frac{\lambda'_0 - \lambda}{\lambda'_0 - \lambda_m}}.$

Designating now the expression  $a^2 - b^2 - 1 = N^2 - 1$  as the merely *refractive*,  $2ab$  as the at the same time *absorptive* part of the index of refraction  $n$ , the former diminishes as we proceed from the right-hand limit  $G'$  of the absorption-streak (at which it attains the value  $+\sqrt{D} = n_0'^2 - 1$ ) to the middle line. At this,  $a^2 - b^2 = N^2 = 1$ , and sinks for the limit-point  $G''$  on the left still more, to  $1 - \sqrt{D} = n_0''^2$ . If, therefore, we imagine the limit-points  $G'$ ,  $G''$  connected with each other by a curve

constructed according to the foregoing law  $N = F(\lambda)^*$ , this curve forms in a certain relation the continuation and complement of the till now isolated branches of the real dispersion-curve; its ordinates increase simultaneously with the wave-length.

The absorptive part, on the contrary, increases from the two limits to the middle; hence everywhere the upper or the lower sign only is to be used.

Passing, finally, from quite feeble to stronger dispersive forces, the consideration of equation (4) gives, on substituting in it  $\frac{\lambda}{n}$  for  $l$ , and solving it according to  $n^2 - 1$  instead of  $n$ , immediately:—

$$n^2 - 1 = -\frac{1}{2} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right) \pm \sqrt{\frac{1}{4} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right)^2 - D}. \quad (14)$$

It divides for the complex zone into the two following,

$$a^2 - b^2 - 1 = -\frac{1}{2} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right),$$

$$2ab = \pm \sqrt{D - \frac{1}{4} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right)^2},$$

and gives, besides  $\frac{m' \rho_0'^2}{m \rho_0^2} = \sqrt{D}$ , more complicated values for  $2(\chi' - \chi)$ . For the middle line particularly this angle always becomes less than  $90^\circ$ .

That the processes here indicated never take place without absorption, is scarcely surprising. *We have, in the occurrence of the difference of phase between the æthereal and corporeal particles, an implicitly communicated coefficient of friction; and the intensity of the absorption † will be exactly proportional to*

\* Of course  $N$  must be distinguished from the actual velocity- or sine-ratio,  $\nu = \frac{v}{\omega} = \frac{\sin e}{\sin r}$  (for perpendicular incidence  $= a$ ), whose difference  $\nu^2 - 1 = \frac{m'}{m} Q^2$ , becoming variable, must be regarded as the *actual* refracting force. *Cf. l. c.* pp. 70, 85; *et infra* sub § 15.

† If, after Cauchy, we put the absorption-factor of the amplitude equal to  $e^{\beta z}$ ,  $\beta = \frac{2\pi}{\lambda} \frac{ab}{\nu \cos r}$ , the abscissa  $z$  being taken vertically, we have, for  $D$  small,

$$e^{-\frac{\pi}{l} \frac{m' \rho_0'^2}{m \rho_0^2} \sin 2(\chi' - \chi) d},$$

where  $d$  denotes the thickness passed through, and  $\beta$  is only negative. I accept this fully, but conditionally—that is, rigorously retaining the preceding signification, and without any connexion with the so-called limit-equations. *Conf. l. c.* p. 69.

$\sin(\chi' - \chi)$ , indifferently whether its sign (and therewith the resulting elliptic polarization) is positive or negative. Under this assumption, the difference of brightness between the edge and middle of an absorption-band becomes greater in proportion as  $D$  is less and consequently the band is narrower.

Moreover the preceding discussions have cleared up many formerly obscure points; and if the approximative formula (9), with corresponding treatment, may be applied to the absorption as well as to the refraction of composite media, on the other hand it can scarcely be any longer doubtful that the phase-difference  $(\chi' - \chi)$  does not, like the "coefficient of ellipticity," diminish towards the interior of the medium, but much rather remains constant, so that in each successive stratum an equal quantity of regular is converted into irregular oscillatory motion.

[To be continued.]

### XLIII. *On Division-Remainders in Arithmetic.*

By W. H. WALENN, *Mem. Phys. Soc.\**

**R**EGARDING the dividend, divisor, quotient, and remainder in an ordinary numerical division as algebraical quantities, the operation may be put in the form of the equation

$$\frac{a}{b} = c + \frac{r}{b}.$$

The expression  $c + \frac{r}{b}$  is a function of  $r$ ; it is also a function of  $c$  and  $b$ , and  $r$  cannot be obtained from it without knowing  $c$  as well as  $b$ . If the division symbolized by  $\frac{a}{b}$  be carried so far that  $c$  is either wholly decimal or partly integral and partly decimal,  $r$  is a decimal less than unity. The following are instances of this:—

$$\begin{aligned} \text{I.} \quad \frac{25}{61} &= 0.409 + \frac{0.051}{61} \quad \dots r = 0.051. \\ \text{II.} \quad \frac{315}{33} &= 9.545 + \frac{0.015}{33} \quad \dots r = 0.015. \\ \text{III.} \quad \frac{12}{7.854} &= 1.527 + \frac{0.006942}{7.854} \quad \dots r = 0.006942. \end{aligned}$$

\* Communicated by the Author, having been read before the British Association, Section A, September 9, 1876.

Thus, in the ordinary way of considering the operation of division, not only is it necessary to know the quotient in order to obtain the remainder, but the value of the remainder is dependent upon the value of the last figure of the quotient. This is shown by the fact that if example I. were carried one step further, the quotient would be 0.4098 and the remainder 0.0022.

If, however, in the equation  $\frac{a}{b} = c + \frac{r}{b}$ ,  $c$  be put out of consideration, if  $a$  and  $b$  be integral, and if successive numerical values (as 1, 2, 3, &c.) be assigned to  $a$ , the corresponding values of  $r$  will also be successive; they will moreover be periodic, having for their period a value equal to  $b$ . For instance, when  $\frac{a}{9}$  is realized mentally in reference to any remainder it may possibly have, the remainder must be one of the numbers or units 1, 2, 3, 4, 5, 6, 7, 8, 9, taking, for convenience, 9 to be the remainder when the dividend is an exact multiple of the divisor, so as to include all instances without the use of the symbol 0.

This is equally true of the general statement  $\frac{a}{b}$ , when  $b$  is any whole number; for the point borne in mind in this way of regarding division is the divisor, in reference to whether it is contained a whole number of times or not in the dividend, quite independent of the quotient. If it be contained a whole number of times, the divisor may be put down as the remainder; if it be not contained a whole number of times, the remainder (in respect of the number of units that make up the divisor) must be one of those units. Considering the remainder to a division in this light, no remainder can be a fractional quantity if the dividend be finite and the divisor integral.

Manifestly the use of looking upon the remainder to a divisor in relation to a function that contains the quotient, as in the formula  $\frac{a}{b} = c + \frac{r}{b}$ , is either to be able to continue the division to the next figure, or to obtain the result of the division itself with rigid and absolute accuracy. Viewed in this light,  $r$  has no other uses and no other properties.

If, however, the quantity  $c$  be put out of consideration, and the remainder be viewed as being made up of units from unity itself to the divisor itself, both inclusive, the case is quite different; the remainder becomes an entity or function which has properties of its own, that not only apply to the particular instance whence it has its being, but which may be used

wherever calculations have value. More than a thousand years ago the Arabians used the *tarazu* or *balance* to check the operations of multiplication and division; the operation called *tarazu* is that of casting out the nines, or ascertaining the remainder to 9 that any given number has, and with these remainders performing the same operations as with the numbers whence they are derived.

Lucas de Borgo, in the fifteenth century, used the remainders to division by 9, as well as to division by 7, obtained for the various data in a numerical calculation (each datum having its corresponding remainder), to check the operations of addition, subtraction, multiplication, and division. This employment of the remainder to a divisor is perhaps the first instance of the use of a corresponding or factitious number or function in computations; for logarithms were invented by Baron Napier long after—namely, in 1614.

Mathematicians of recent times have extended the isolated observations of Lucas de Borgo, and have proved the still more general principle that all direct and some inverse operations upon the remainders to divisors are respectively analogous to the same operations upon the dividends. This now appears evident from the parcelling out of numerical values which division by a constant divisor affords. The difference between these remainders, as factitious numbers or functions, and logarithms, is, that in the first case the same operation—but in a shorter and more condensed form—is used to obtain a result; whereas, in the second case, the factitious numbers are capable of being dealt with by an operation one degree lower in the scale of operations. Moreover, in the case of remainders, the application is possible in consequence of their periodicity, and the original result of the calculation is not obtainable, but is simply analogized; whereas the logarithms being continuous functions, give results quite parallel to those of the intended calculation, and therefore translatable into the answer by suitable calculations or references.

Looking upon these remainders as functions of numbers, it is important that their use in performing analogous calculations to a primary one should be recognized and made possible by means of Tables, also that a method should be obtained for their easy discovery.

From what has been published in the Transactions of the Sections of the British Association for the Advancement of Science (see the 'Report' for 1870), it appears that the numerical theorem therein put forth by the author of the present paper gives a means of ascertaining the remainder to a divisor without knowing any thing of the value of the quotient. This

theorem is that  $(10-\delta)t+u$  has the same remainder to division by  $\delta$  that  $10t+u$  has,  $10t+u$  being a given number in terms of its digits, and  $\delta$  being any whole number; this formula, which is a function of  $\delta$ , but not of the quotient of the division, may be expanded to the form

$$(10-\delta)^{n-1}a_n + (10-\delta)^{n-2}a_{n-1} + \dots + (10-\delta)^2a_3 + (10-\delta)a_2 + a_1.$$

This form is well adapted for use in general calculations. Since  $a_1$  is the extreme right-hand digit of any number, it is only necessary that the dividend should be finite;  $a_1$  may either be the unit digit or a digit on the right hand of the decimal point. From this it appears that although the divisor must be integral, the dividend may be fractional. That  $(10-\delta)t+u$  and  $10t+u$  have the same remainder to  $\delta$  may be seen from the fact, deduced by multiplication, that

$$(10-\delta)t+u=10t+u-\delta t.$$

In the former paper the name *unitation* is assigned to the above operation upon a given number, which yields the remainder or *unitate* to a chosen divisor or *base*. The function called the unitate of  $x$  to the base  $\delta$  is symbolized by  $U_\delta x$ . Then

$$U_\delta x = (10-\delta)^{n-1}a_n + (10-\delta)^{n-2}a_{n-1} + \dots + (10-\delta)^2a_3 + (10-\delta)a_2 + a_1,$$

$$\text{if } x = 10^{n-1}a_n + 10^{n-2}a_{n-1} + \dots + 10^2a_3 + 10a_2 + a_1.$$

The function  $U_\delta(-x)$  is easily derived from  $U_\delta x$ . For  $-x$  may have  $\delta$  added to it without altering the value of  $U_\delta(-x)$ . Hence  $U_\delta(-x) = U_\delta(\delta-x)$ ; that is,  $U_\delta(-x)$  is the complement of  $x$  to  $\delta$ . For instance,  $U_9(-3) = U_9(9-3) = 6$ . Thus, for every value of  $U_\delta(-x)$  there is a corresponding value of  $U_\delta x$ , and for every value of  $U_\delta x$  there is a corresponding value of  $U_\delta(-x)$ .

The function  $U_\delta\left(\frac{x}{y}\right)$  may be investigated by means of its equivalent  $U_\delta\left(x \cdot \frac{1}{y}\right) = U_\delta(x \cdot y^{-1})$ . In the case of  $\delta$  being a prime number, all the values of this function are integral, excepting when  $y=\delta$ ; as  $U_{11}\frac{1}{3}=4$ ,  $U_{11}\frac{2}{3}=8$ . When  $\delta$  is not a prime number,  $U_\delta\frac{1}{y}$  is only integral when  $U_\delta y$  is not equal



to  $\delta$ , or not a submultiple thereof; for instance,  $U_9 \frac{1}{6} = \frac{1}{6}$ . The laws of reciprocals should be satisfied by unitates; thus  $U_{11} 3 \times U_{11} \frac{1}{3}$  should be equal to unity, and accordingly

$$U_{11}(3 \times 4) = U_{11} 12 = 1.$$

The function  $U_\delta x^n$ , in the case of  $\delta$  being a prime number and  $n$  being the variable, recurs in  $\delta-1$  terms. When  $\delta$  is not prime, the period of recurrence is less than  $\delta-1$ . When  $x$  is the variable and  $n$  an even number, it recurs in  $\delta$  terms, and  $\delta-1$  of those terms are arranged in groups, the latter half of each group being the same as the first half but in inverse order; thus

$$U_{11} x^4 = 1, 5, 4, 3, 9; \quad 9, 3, 4, 5, 1; \quad 11; \quad 1, 5, 4, 3, 9; \quad \&c.$$

The function  $U_\delta x^{-n}$  behaves itself like  $U_\delta x^{-1}$  in respect of its values being integral or fractional.

The function  $U_\delta x^{\frac{1}{n}}$  may sometimes be ascertained in whole numbers from continuing the series  $U_\delta x^{mn}$ . When  $\delta$  is of the form  $p^n - x$  ( $n$  and  $x$  being whole numbers, and  $x^{\frac{1}{n}}$  or  $\sqrt[n]{x}$  being irrational),  $U_\delta x^{\frac{1}{n}}$  is finite and integral. In unitation, as in some other numerical operations, there may be  $m$   $m$ th roots,  $n$   $n$ th roots, and so on.

Taking the function  $U_{10^n} x$  as true when  $x = \sqrt[n]{q}$  (an irrational quantity), it can be proved to be possible to assign the values of the extreme right-hand figures of some incommensurable quantities. For instance,

$$U_{10} \sqrt{5} = 5, \quad U_{100} \sqrt[3]{29} = 9, \quad U_{100} \sqrt[3]{13} = 17.$$

The formula  $U_\delta(-x) = U_\delta(\delta-x)$  may be applied to functions of  $\sqrt{-1}$ ; thus,  $U_\delta(a+b\sqrt{-c}) = U_\delta(a+b\sqrt{\delta-c})$ . In this manner  $\sqrt{-1}$  in unitates may be expressed as a quantity that is not imaginary, without the use of geometry.

The investigation of the function  $U_\delta x$  has not proceeded beyond this point; but several applications of its properties have been made.

Checking tables and calculations that can be finited is among the most practical applications. All divisions to be tested are obliged to be completely finished by obtaining all the figures of the quotient together with the remainder; in this case the division of unitates may be performed in the

same manner as ordinary division, reducing the values from time to time. For instance, to obtain

$$U_9 \frac{269}{122} = U_9 \left( 2 \cdot 2 + \frac{0 \cdot 6}{122} \right),$$

$$U_9 \frac{269}{122} = \frac{8}{5} = 1 + \frac{3}{5} = 1 + 3 \times 2 = 7;$$

and

$$U_9 \left( 2 \cdot 2 + \frac{0 \cdot 6}{122} \right) = 4 + \frac{6}{5} = 4 + 6 \times 2 = U_9 16 = 7.$$

Many theorems that, treated in the ordinary way, involve the cumbersome notation of algebra used to deal with scales of notation, can be investigated by the simplest means. For instance, the proof that the addition of the digits of a number casts out the nines of that number is manifest from the substitution of 9 for 8 in the general formula of unitation given above; for in that case each term has 1 for its coefficient. In the function  $U_{11}x$  the coefficients are, beginning with the unit, +1 and -1 alternately.

These relations of  $U_9x$  (generally written  $Ux$  as being the most common of all the systems of unitation) and of  $U_{11}x$ , give the following singular property, an extension of a well-known fact. If dots or marks be made at equal intervals over the digits of a given number, or part of a given number, and (having regard to the relative position of the marked digits in the number) if the marked digits be transposed among themselves, the remainder obtained by the subtraction of the transposed number from the original number—or *vice versâ*, according to which number is the greatest—will be divisible by a number composed entirely of ones, as 1111, also by some other number composed entirely of nines, as 9999, the number of ones or of nines in the divisor being  $a+1$ , if there be  $a$  intervals. For instance,

$$5 \ 8 \ 2 \ 1 \ 2 \ 3 \ 1 - \overset{\text{I}}{1} \overset{\text{II}}{2} \overset{\text{III}}{3} \overset{\text{IV}}{5} \overset{\text{V}}{8} \overset{\text{VI}}{2} \overset{\text{VII}}{1} = 4 \ 5 \ 8 \ 5 \ 4 \ 1 \ 0,$$

and 4585410 is exactly divisible by 111 and by 999.

An easy and practical method of constructing decimal equivalents for reciprocals is as follows:—Take a reciprocal with any number for its denominator that has 9 for its unit figure, and employ as a multiplier a number greater by 1 than that represented by the remaining figure or figures. In  $\frac{1}{49}$  5 is the multiplier. Beginning with 1, this multiplier is used to obtain the next figure towards the left from the previous one as from a multiplicand, adding-in the tens digit or other digits

that may be carried from the previous product. In this way the decimal equivalent for  $\frac{1}{29}$  is found to be

$$0\cdot03448,27586,20689,65517,24137,931,$$

3 being the multiplier. Since  $\frac{1}{7} = \frac{7}{49}$ ,  $\frac{1}{17} = \frac{7}{119}$ , &c., this plan can be used to give the figures of reciprocals that have 7 for the right-hand figure of their period; the operation in this case is begun with 7 instead of 1. By similar means, other decimal forms can be constructed; the forms that can be decimally wrought out by this means are  $\frac{1}{10a+9}$ ,  $\frac{1}{10a+7}$ ,  $\frac{1}{10a+3}$ , and  $\frac{1}{10a+1}$ . The other decimal forms of reciprocals are either finite, or they may be derived from the form  $\frac{1}{10a+9}$ .

A method, which is sometimes easier than the above, is to use the multiplier as a divisor, beginning with the last figure of the period, working towards the right hand, and taking each figure of the quotient as it is obtained as the figure to be brought down for the unit figure of the dividend at each operation. The work to obtain  $\frac{1}{7} \left( = \frac{7}{49} \right)$  by this method is:—5 into 7, 1 time and 2 over; 5 into 21, 4 times and 1 over; 5 into 14, 2 times and 4 over; 5 into 42, 8 times and 2 over; 5 into 28, 5 times and 3 over; 5 into 35, 7 times and 0 over; 5 into 07, 1 time; and so on, giving the digits (7)  $\cdot1428571$ , &c.

In reference to unknown quantities, the power of unitation to ascertain the remainders to divisors thereof may be shown by an example. The unknown quantity must be connected with a known quantity by definite laws. If, for instance, it be required to take the 647th power of 256 and to ascertain the remainder, to the divisor 7, of that high number, the following is the work, taking into account that the function  $U_7x$  has 6 for its repeating period:— $U_7256=4$ , and  $U_6647=5$ ; then  $U_74^5=2$ . Therefore  $U_7256^{647}=2$ .

The function  $U_\delta f$ ,  $f$  being a figurate number, has, to some extent, been investigated.  $U_9f$  does not repeat, according to any law that has yet been discovered; but groups of these unitates repeat; those that have been examined repeat either after 9 terms or after  $9n$  terms.  $U_{11}f$  being formed by a square arrangement of the numbers as they are obtained, repeats after 11 terms in the horizontal as well as in the vertical

direction. Taking the first and representative square (which has only ones for the numbers in its periphery), the diagonal and the triangular half which has its apex in the lower right-hand corner, its hypotenuse being the diagonal, has 11 for all its contained values. As  $U_7f$  behaves itself similarly to  $U_{11}f$  in this respect, it is probable that prime numbers for the value of  $\delta$  favour this formation. The unitation square of  $U_6f$  is in the margin.

1	1	1	1	1	1	1
1	2	3	4	5	6	1
1	3	6	4	3	3	4
1	4	4	2	5	2	6
1	5	3	5	4	6	6
1	6	3	2	6	6	6
1	1	4	6	6	6	6

Of the eight perfect numbers that are known (calling the series  $p$ ), a few of the unitation functions are :—

$U_2p=2, 2, 2, 2, 2, 2, 2, 2.$	$U_7p=6, 7, 6, 1, 1, 6, 1, 7.$
$U_3p=3, 1, 1, 1, 1, 1, 1, 1.$	$U_8p=6, 4, 8, 8, 8, 8, 8, 8.$
$U_4p=2, 4, 4, 4, 4, 4, 4, 4.$	$U_9p=6, 1, 1, 1, 1, 1, 1, 1.$
$U_5p=1, 3, 1, 3, 1, 1, 3, 3.$	$U_{10}p=6, 8, 6, 8, 6, 6, 8, 8.$
$U_6p=6, 4, 4, 4, 4, 4, 4, 4.$	$U_{11}p=6, 6, 1, 10, 6, 10, 4, 6.$

Algebraical geometry applied to unitation yields some striking results, especially when polar coordinates are used. The polygons obtained by thus setting out the values of functions of  $U_\delta x_1$  in reference to the corresponding and consecutive whole-number values of  $U_\delta x$  itself, represent graphically those functions in a convenient form.

The values of functions as given above, also the properties of unitates that are put forth, must be regarded as results given as simple statements; the proofs of these and of some of the applications of the general formula, together with Tables and methods of calculating unitates, may be found in a series of articles now publishing in the *Philosophical Magazine*. The published papers are dated:—I. November 1868; II. July 1873; III. May 1875; IV. August 1875; V. December (Supplement) 1875; VI. June (Supplement), 1876.

Unitates manifest themselves as functions which may be found throughout the whole domain of quantity; they are in some instances interpretable when the quantity from which they are derived is but imperfectly known.

74 Brecknock Road, N.  
August, 1876.

XLIV. *On a Model illustrating Mechanically the Passage of Electricity through Metals, Electrolytes, and Dielectrics, according to Maxwell's Theory.* By OLIVER J. LODGE, B.Sc.\*

[Plate III.]

§ 1. **T**HE obstructions which an electric current meets with in its passage through a medium are of two kinds, viz. opposition electromotive force and resistance proper. The first, since it does not necessarily change sign nor vanish with the current, has a tendency to produce a countercurrent. The second opposes the current with a force which is always proportional to the current and always acts against it; it can therefore produce no reverse effects.

Since the laws of the flow of electricity are in most respects the same as those of an incompressible fluid moving in a closed circuit, we may represent a current of electricity in a conducting medium by a flexible inextensible endless cord circulating continuously over pulleys. When the cord is at rest in the conducting medium, it is no more (and no less) to be called electricity than an ordinary copper wire is to be said to contain electricity at rest; but when it is in motion it is to be called an electric current, and the strength of the current through unit area of the conductor is to be measured by the quantity of cord which passes any fixed point in a second; in other words, the strength of the current is proportional to the velocity of the cord. The cord can be made to move by applying to any part of it, in the direction of its length, a force which corresponds to electromotive force, and which can be generated in various ways, as by a winch, a weight, or an elastic string.

To represent the resistance of conductors, the cord may be made to rub against rough surfaces; and to represent opposition electromotive force, the motion of the cord may be made to stretch or to squeeze elastic bodies, thereby setting up a tension in them which shall not only oppose the motion, but shall also have a tendency to force the cord in the reverse direction. The difference of potential between any two points of a circuit is represented by the difference between the tensions of the cord at the two points; a greater tension is to represent negative potential, while a less tension or a pressure is to represent positive potential.

If the cord passes through a body, and if by any means we can make that body include more of the cord than its normal allowance, then that body is said to be positively charged with

\* Communicated by the Author, having been read before Section A of the British Association in Glasgow, September 1876.

electricity ; if, on the other hand, we can abstract some of the cord from the body, it is negatively charged. The *capacity* of the body will be measured in the ordinary way by the excess of cord which is got into or out of the body, divided by the difference of potential required to get it in or out.

*Model to illustrate the properties of Dielectrics.*

§ 2. Now let us apply these considerations to the construction of a model which shall represent the charging of a Leyden jar. In fig. 1 (Plate III.) A B C D represents the cord, passing over four pulleys, A, B, C, D, and through eight buttons, 1, 2, 3, &c., which represent eight strata in the dielectric of a Leyden jar or other accumulator ; these buttons are supported on elastic strings, and they grip the cord tightly so as only to allow it to slip through them very slowly and with considerable resistance, if at all. Continuous electromotive force is represented as being applied to the cord at D, in the shape of a weight W, tending to turn one of the pulleys on which the cord runs ; and there is a contact-breaker at C, represented by a screw S, which is able to clamp the cord, or in other words to make the resistance of the external circuit infinite. The inner coating of the jar may be represented by the space between A and the first button ; the outer coating by the space between the last button and B. The part A B C D of the circuit, since it contains ordinarily next to no resistance, represents very good metallic conduction.

Suppose initially that the cord is free at C, and that the weight W has never been applied ; all the elastics will be vertical, the buttons will be at the same distance from one another, the distance A 1 equal to the distance B 8, and there will be no excess of tension in any part of the cord. This represents an uncharged Leyden jar, its inner and outer coating connected by a metallic wire.

Introduce into the circuit at D an electromotor (like W). The potential towards A rises, the potential towards C falls, and the cord begins to move from A to B. The first effect of this motion is to displace the buttons all the same distance to the right—to such a distance, namely, that the sum of the tensions in their elastics is equal to the weight W. Then the cord stops. The dielectric is polarized ; and its condition is shown in I, fig. 2. Electricity has been displaced in it in the direction of the electromotive force ; the inner coating A 1 has gained cord ; it is charged positively ; the outer coating B 8 is charged negatively ; pressure is exerted on the left face of every button, and tension on its right face ; that is, every stratum is electrified positively on the side *from* which the

electricity came, negatively on the other side. The potential gradually falls from A to B, *i. e.* the tension in the cord gradually increases as we pass along it from A towards B. The dielectric is in a state of strain represented by the stretching of the elastics; and it is tending to produce a countercurrent, *i. e.* to drive the cord back again.

§ 3. Now clamp the cord by means of the screw S, take off W, and leave the whole to itself for some time. Several things may happen.

1st. The buttons may remain exactly where they were, which represents a dielectric of perfect insulating power: the jar will in this case remain charged for any length of time, and the whole charge can be got out of it at any time by unclamping the cord. In the symbols of § 8,

$$r=\infty, \quad p=0, \quad \frac{df_1}{dt} = \frac{df_2}{dt} = \dots = u, \quad f_1=f_2=\dots=Q, \quad \sigma=0.$$

As soon as the screw S is unclamped all the buttons spring back, overshoot their mean position, and execute a rapid series of decreasing oscillations, carrying the cord with them backwards and forwards until they come to rest. The discharge of a jar, then, is not a simple passage of electricity in one direction only, but is a rapid series of discharges in alternately opposite directions, which last until the energy stored up as strain in the dielectric is all converted into heat by the resistance of the circuit. This succession of alternately opposite sparks Feddersen\* has observed experimentally by looking at the apparently simple spark of a Leyden jar in a revolving mirror. If it were possible to close the circuit so rapidly that very little of the discharge should have taken place before good metallic contact was made, the succession of opposite currents would probably last quite an appreciable time. They might perhaps be detected by discharging the jar through a galvanometer and an electro-dynamometer at the same time; the relation between their indications would probably be different in this case from what it is when either a continuous current or a simple one-directioned discharge is sent through them both.

2ndly. The buttons may all slowly and *equally* slide back towards their normal position. This will happen if the relation between their friction on the cord and the elasticity of their strings is the same for all. The model will in this case represent a homogeneous dielectric with feeble conducting-power.

\* Pogg. *Ann.* vol. ciii. Feddersen saw the spark drawn out into a succession of bright parallel lines at continually increasing intervals. Maxwell's theory, equation (2), § 9, appears to make the intervals all equal, but only on the erroneous assumption that the circuit is devoid of resistance.

If left to itself long enough, such a jar would be found completely discharged; and since all the buttons would have preserved their original distance from one another, there would be no internal charge in any part of the jar; and residual charge is impossible. The values of the symbols of § 8 for this case are

$$f_1=f_2=\dots, \quad p_1=p_2=\dots, \quad \frac{r_1}{\kappa_1}=\frac{r_2}{\kappa_2}=\dots=\frac{f}{p}, \quad \sigma=0;$$

and after charging it by means of an electromotive force  $E_0$  and leaving it for any time  $t$ , the whole charge can be got out of it at once, and its amount will be

$$CE_0e^{-\frac{t}{CR}}.$$

3rdly. Some of the buttons may be smoother or may have stronger elastics than others; and in such case they will slide back more quickly than the others and will represent strata of comparatively high conducting-power. Thus in fig. 1, or in II (fig. 2), 3, 5, and 6 are such buttons, especially 5; while 1, 4, 7, and 8 have remained pretty steady without slipping. What, then, is the condition of the dielectric in this state? The slipping back of some of the buttons will have relieved the tension on the cord, in other words, will have diminished the difference of potential of the two coatings; but no motion of the cord has taken place either way because of the clamp at C. In the figure the buttons 1 and 8 have remained tight on the cord; consequently the charges on the two coatings happen to have remained constant; but charges have appeared in between the strata. For instance, between 3 and 4 there is too much cord; the dielectric is there positively charged; between 4 and 5 it is negatively charged. The difference of potential between the two faces of the stratum 5 has almost vanished.

§ 4. Now unclamp the screw S, *i. e.* make metallic contact between the two coatings: a rush of electricity takes place; the buttons spring back, and after a few oscillations take up the position of equilibrium shown in III, fig. 2. They have in fact been all displaced the same distance to the left; and so those which (like 3, 5, and 6) had, before discharge, slid almost to their mean position are now displaced beyond it, and the strain in their elastics is acting in the reverse direction to what it did before; consequently the elastics of 1, 2, 4, 7, and 8 have not been able quite to resume their normal length, but they remain in such a state of strain as to exert as much stress one way as the elastics of 3, 5, and 6 are exerting in the other direction. The jar then appears discharged, but we have not yet got the whole charge out of it. Screw down S again and wait.



The slippery buttons 3, 5, and 6 will again gradually slide back almost to their mean position, the tension which their elasticities exerted towards the right will be relieved, and we shall presently have an unbalanced tension in the same direction as at first: this is the "residual charge;" and the state of things is represented in IIII, fig. 2. On unscrewing S a second motion of the cord will take place, and the buttons will take up a fresh position of temporary equilibrium. The numbers annexed to the four cases I, II, III, and IIII indicate the electromotive force, or the stress on the cord, in each case. Thus when first charged it is 24; after standing some time it falls (say) to 17; immediately after the first discharge it is of course 0; and after again waiting some time it rises to 3 in the same direction as before, again to fall to zero when the second discharge occurs. Still, however, the jar is not wholly discharged, because the slippery buttons will again have been displaced beyond their mean position and will slide back again; so it will take an infinite number of momentary contacts completely to discharge the jar, unless indeed *every stratum* allows a little slipping to go on.

This is Professor Clerk Maxwell's theory of a composite dielectric, which he explains in art. 328 of his 'Electricity and Magnetism,' and which he also illustrates mechanically in art. 334.

§ 5. If any button is *perfectly* smooth, it does not get displaced at all, and it therefore takes no part in the action. It would represent a film of gold leaf lying inside the glass of the jar parallel with the coatings; and it would become oppositely charged with electricity on its two sides—negative on the side facing the positively charged coating, and positive on the other; for the other buttons would evidently move closer up to it on one side, and away from it on the other.

This, together with the preceding, really illustrates the whole of the phenomena ordinarily spoken of as *charge in conductors* according to the views of Faraday and Maxwell; for to electrify any conductor by induction, you place it inside a polarized dielectric (usually the air); and the opposite electricities appear on its surface exactly as in the above piece of gold leaf; while to "charge a conductor" with one kind of electricity you use the conductor to coat one face of a polarized dielectric, which is done by judiciously relieving its state of strain on one side only of the conductor you wish to charge. Conductors then are to be looked upon statically as mere interruptors in the continuity of a dielectric medium; and if the medium is polarized, charge appears at the surface of discontinuity. Regarded in this way, the analogy between electrification and magnetization is very close; and we can no more get positive

electricity detached from an equal quantity of negative than we can get a north pole detached from a south: only in one case steel is the polarized medium, in the other case air.

§ 6. Instead of making momentary contacts as in § 4, we might leave the screw S up so as to obtain a feeble continuous current from the dielectric by reason of the gradual slipping of some of its buttons, till it is completely discharged. The strength of this current at any instant Maxwell calculates in art. 330.

Again, after having charged the dielectric, instead of removing W, we might leave it on and not clamp the cord. This would represent a jar permanently connected with an electromotor maintaining a constant difference of potential. If none of the buttons slipped on the cord, nothing would in this case happen after the first charge. If all the buttons slipped equally, we should get a feeble continuous conduction-current through the dielectric after the first rush due to electrical displacement had subsided. But if some of the buttons remained firm on the cord while others slipped, we should have a slight continuous current not purely of conduction, nor purely of displacement, but of both combined. There would be conduction through the slipping buttons, and there would be displacement of the tight ones with increasing tension in their elastics to make up for the relaxation of the others; and this might go on for a long time, the current continually getting weaker. This is actually observed in the dielectric of submarine cables (cf. Maxwell, art. 366). The insulating material of a cable, in fact, consists of layers of different materials (*gutta percha*, Chatterton's compound, &c.) laid in strata over the conductor, as if these residual-charge effects were exactly what was wanted to make a good cable. I suppose that they do not exert any detrimental influence on the use of a cable, the method of signalling being what it now is; but if it were desired to lessen these effects, it might be done by rendering the dielectric as homogeneous as possible, first of all by making it of only one material, and secondly by laying this material on the conductor by some process which shall not necessarily develop in it a stratified structure as the present method does.

#### *Disruptive Discharge.*

§ 7. When the electromotor is acting on the dielectric in the way last described, the strain in some of the elastics may become so great as to break some of them; we should then have partial or complete disruptive discharge, and the subsequent resistance of the dielectric would be much diminished.

Such disruptive discharge as this represented by a snapping of the elastics is analogous to the rupture of solid dielectrics like glass. In fluid dielectrics like air, the disruptive discharge may be better represented by a slipping of the pins to which the elastics are fastened along a groove parallel with the cord: at each slip the cord makes a bound forward; but no permanent damage is done. (If at any one point a slip takes place, the extra stress thrown on to the others is liable to make them give way too.) A discharge of this sort is a kind of electrolysis, and may be supposed to go on slowly in glass at a high temperature. It would be accompanied by a polarization of the electrodes, as we shall see later (§§ 13 and 15). Discharge in a vacuum-tube seems to be of this nature. If partial disruptive discharge occurs at points inside a dielectric, it will in general give rise to internal charge, even though the dielectric be of uniform conductivity, because the buttons will not preserve their original distances from one another on the cord.

*Maxwell's Theory of a Composite Dielectric.*

§ 8. Let us now see how the model must be made in order to agree accurately with the dielectric, as investigated in art. 328, vol. i. of Maxwell's 'Electricity.'

It is there supposed, "for the sake of simplicity, that the dielectric consists of a number of plane strata of different materials and of area unity, and that the electric force acts in the direction of the normal to the strata." The row of buttons in our model (fig. 1) will represent this collection of strata; and we shall, for simplicity, consider the normal distance between adjacent buttons in the row as unity. The thickness  $a_1$ ,  $a_2, \dots$  of any stratum is then equal to the number of buttons (or molecules) which compose it; and the whole thickness of the dielectric  $a_1 + a_2 + \dots + a_n$  will equal  $n$ , the whole number of buttons. Since all the buttons in any one stratum have exactly the same properties, we may if we like consider each button to represent an entire stratum, and take  $a_1 = a_2 = \dots = 1$ ; and this is what we shall virtually do in what follows. The other symbols employed in the investigation are the following, with their meanings affixed (1st, the electrical one after Maxwell, and 2nd, the corresponding meaning expressed in terms of the mechanical model):—

$X_1$ ,  $X_2$ , &c. [the resultant electrical force within each stratum]. The tension in the elastic of each button if the buttons are unit distance apart, or, more generally, the difference between the pressures on the corresponding faces of two consecutive buttons divided by the distance between them.

$p_1, p_2$ , &c. [the current due to conduction through each stratum]. The rate at which the cord slips through each button.

$f_1, f_2$ , &c. [the electric displacement]. The distance of each button\* from its mean position.

$u_1, u_2$ , &c. [the total current through each stratum, due partly to conduction and partly to variation of displacement]. The rate at which the cord passes by a point in each stratum fixed in space.

$r_1, r_2$ , &c. [the specific resistance referred to unit of volume]. The coefficient of "friction" (see § 10) between the cord and each button.

$\kappa_1, \kappa_2$ , &c. [the coefficient of electric elasticity, which is equal to  $\frac{4\pi}{K}$ , where  $K_1, K_2$ , &c. are the specific inductive capacities of the several strata (see Maxwell, art. 60)]. The "coefficient of elasticity" of each elastic, or the force tending to replace a button when displaced unit distance from its mean position (see § 10).

E [the electromotive force due to a voltaic battery placed in any part of the external circuit]. The force applied at an external point such as D (fig. 1) to move the cord, or the tension in the part of the cord between D and C minus that in the part between D and A.

$u$  [the current in the external circuit and battery]. The rate at which the cord passes over the fixed pulleys.

Q the total quantity of  $\left\{ \begin{array}{c} \text{electricity} \\ \text{cord} \end{array} \right\}$  which has passed through the external circuit up to the time  $t$ .  $Q = \int_0^t u dt$ .

$R_0$  [the resistance of the battery with connecting wires]. The drag on the cord in the external circuit, including electromotor at D, pulleys, and any other rubbing surfaces as S.

\* In the model, then, electric displacement in a dielectric is accompanied by displacement of matter. It may possibly be so also in fact; and if so, on suddenly charging or discharging a condenser, a sudden kick might be observed, the body as a whole being thrown forward with a momentum equal and opposite to that of its displaced particles. But this is not at all necessary, because one does not see to what the other ends of the elastics can be attached except to another set of buttons which get displaced in the opposite direction by a negative current simultaneous with, and equal to, the recognized positive current. (This set of buttons must be understood to take the place of the fixed beams employed in the model for convenience.) As, however, there is no reason apparent why the masses of the two sets should be equal, a kick might be observed equal to the difference of the two momenta; at any rate it is worth looking for.

$\sigma_{12}$  [the surface-density of electricity on the surface which separates the first and second strata]. The length of cord in excess of the normal length, which exists between the first and second buttons.

R the regular *Ohmic* resistance of the dielectric, or

$$a_1r_1 + a_2r_2 + \dots + a_nr_n.$$

C the capacity of the dielectric measured instantaneously, *i. e.* without allowing time for any slipping between cord and buttons, or  $\frac{dQ}{dE}$ ; it equals  $\frac{1}{a_1\kappa_1 + a_2\kappa_2 + \dots + a_n\kappa_n}$ .

§ 9. Maxwell then goes on to consider what happens in any one stratum (say, the first) when the electric forces are brought to bear on the dielectric.

By Ohm's law we must have

$$a_1X_1 = a_1r_1p_1. \quad (1)$$

Maxwell's theory assumes

$$X_1 = \kappa_1 f_1; \quad (2)$$

and by definition of total current,

$$u_1 = p_1 + \frac{df_1}{dt}. \quad (3)$$

Also evidently

$$\sigma_{12} = f_2 - f_1, \quad (4)$$

and

$$\frac{d\sigma_{12}}{dt} = p_1 - p_2. \quad (5)$$

These equations being given, certain consequences follow, of which the most obvious are

$$u_1 = u_2 = \dots = u_n = u, \quad (7)$$

and

$$E = a_1X_1 + a_2X_2 + \dots, \quad (10)$$

while the most important is a relation between  $E$  and  $u$ , which is obtained by substituting in (3) the values of  $p$  and  $f$  from (1) and (2), and then adding up in accordance with (10) thus,

$$E = (a_1D_1 + a_2D_2 + \dots)u, \quad (11)$$

where  $D_1$  stands for the operator  $\left(\frac{1}{r_1} + \frac{1}{\kappa_1} \cdot \frac{d}{dt}\right)^{-1}$ .

Equations (4) and (2) give us

$$\sigma_{12} = \frac{X_2}{\kappa_2} - \frac{X_1}{\kappa_1},$$

which shows that internal charges will appear unless the stretch-

ing forces are proportional to the elasticities of the several threads, or unless these elasticities are infinite (as they practically are in the case of metals). If an accumulator be charged by means of an electromotive force  $E_0$  continued so long that the  $p$  become uniform and constant and equal to  $\frac{E_0}{R}$ , the last equation may be written, by help of (1),

$$\sigma_{12} = \left( \frac{r_2}{\kappa_2} - \frac{r_1}{\kappa_1} \right) p, \quad . \quad . \quad . \quad . \quad (19)$$

which proves that there will be no internal charge even in this case if the  $r$  are proportional to the  $\kappa$  (cf. case 2, § 3).

After having charged the jar with this long-continued force  $E_0$ , discharge it instantaneously, and then leave it to itself for a time  $t$  with  $S$  screwed down. The difference of potential between the two coatings, which was at first zero, rises by irregular slipping of the buttons; and its value  $E$  at the end of any time is given by (24), which we may write

$$\frac{E}{E_0} = \left( \frac{a_1 r_1}{\Sigma(ar)} - \frac{a_1 \kappa_1}{\Sigma(a\kappa)} \right) e^{-\frac{\kappa_1}{r_1} t} + \left( \frac{a_2 r_2}{\Sigma(ar)} - \frac{a_2 \kappa_2}{\Sigma(a\kappa)} \right) e^{-\frac{\kappa_2}{r_2} t} + \dots \quad (24)$$

A quantity of electricity  $EC$  can at the same time be got out of the jar; and this is the residual charge.

*Conditions required of the Model in order to be an accurate representation.*

§ 10. The conditions which the model does not naturally satisfy are (1) and (2). Condition (2), however, that the restoring force shall vary with the displacement, is nearly satisfied by fig. 1 for small displacements; and it can be made accurately true by passing each elastic through a smooth fixed ring placed in the line of its mean position at a distance from the fixed pin equal to the natural length of the unstretched elastic. Then (2) is satisfied by the model; and each button, if pulled aside, would execute a succession of simple harmonic motions about its mean position.

To satisfy condition (1) is not quite so easy. Electrical resistance is very different from ordinary friction, though they are to some extent analogous. The smallest electromotive force is sufficient to produce *some* current against any resistance however great, and the current increases proportionally to the electromotive force from zero upwards; but friction is a discontinuous quantity. For small values of force applied to move a body, the friction is exactly equal to the force, and the body does not move; but as the force increases, at a certain critical point the friction suddenly falls from the value

of the force at that instant to something less, and at this value it continues constant as long as the body continues to move. This appears nearly what is wanted to imitate disruptive discharge in a vacuum-tube (see Mr. Varley's experiments, Maxwell, art. 369); but in the case of a metallic conductor or an ordinary dielectric, the cord is required to slip through the buttons, if it slip at all, even when the force is slight; and the rate of slip is always to be proportional to the force. This might be roughly attained in the model by making each button of a short piece of india-rubber tube well greased inside, and by attaching the ends of its elastics to a ring fixed on the tube near one end, so that when it was displaced the pull on its elastics should make it grip the cord more tightly, and thus keep the ratio, between the force pulling the cord through and the rate at which it came through, roughly constant. But it would be probably unnecessary to do this; for by making very smooth and well-oiled buttons pinch the cord tightly, one could arrange that the cord should be able to ooze slowly through them without sticking; and then the residual-charge phenomena would be imitated by the model with sufficient closeness. Moreover I have reason to think that friction between cord and metal is not independent of velocity.

#### *Metallic Conduction.*

§ 11. In order to make the model represent the flow of electricity through a metallic conductor, we have only to make the resistances  $r$  small and the elasticities  $\kappa$  very great. We thus approximate to a set of smooth buttons supported on rigid fixed rods, as shown in fig. 3. In this case the displacements are zero, the whole current is a conduction-current; and there is no tendency to a reversal of the current by reason of an opposition electromotive force. In symbols,

$$f=0, \quad p=u, \quad \sigma=0, \quad \kappa=\infty, \quad E=Ru.$$

To make it exhibit electrolytic conduction (or rather *convection*), we shall see in § 13 that the rods supporting the buttons must still be rigid, but that the pins by which they are attached to the beams must slide in a long groove parallel with the cord.

#### *Submarine Cables.* (See also § 6.)

§ 12. If a circuit consists of a simple wire hung in space, the passage of electricity through it will be represented in the model by a single endless cord; and the cord may be absolutely inextensible, so that the propagation of potential takes place instantaneously; or possibly it may be more in accordance with Maxwell's theory to say that the cord is so very

nearly inextensible that impulses are propagated along it with the velocity of light. But if the wire, instead of hanging in free space, is coated with some insulating material and imbedded in the ground, the propagation of potential goes on comparatively slowly. There are, in fact, besides the simple circuit straight through the wire and back through the earth, a multitude of lateral circuits, each represented by a separate cord, one for each unit area of the dielectric. When an electromotive force, say, a negative potential or a pull, acts at one end of such a compound circuit, its first effect is to pull out those cords which offer least resistance; in fact the current is split among the several circuits in the inverse ratio of their respective resistances. The direct and longest cord feels a slight instantaneous pull indeed; but it is only slight until the cords through the dielectric have been pulled and a tension set up all along the line. While this is going on, the pull on the direct cord is gradually increasing. It would be possible to represent all this by a sufficiently elaborate model; but the action may be imitated with only a single cord if it be arranged zigzag on pulleys supported by elastic strings, as shown in fig. 4. A sudden pull at one end of such a cord will stretch the elastics of the near pulleys; the strain so caused will stretch the next, and so on; and thus the impulse will be transmitted along the cord, getting, however, weaker and weaker as it advances. A still simpler imitation of the effects observed would be obtained by making the direct cord itself elastic; but such an illustration would have very little analogy with the process really going on.

### *Electrolytic Conduction.*

§ 13. A model intended to represent the passage of a current through an electrolyte is shown in fig. 5; but it is not intended to be actually constructed\*. In this figure the buttons are supported by rigid rods; but instead of these being screwed to beams as in fig. 3, they are attached to rings which slide freely on two glass rods *a* and *b*. The additional piece *Z* is a knife-edge mounted on elastic pillars or spiral steel springs, and placed close to the cord, so that its sharp edge is in the path of the buttons if they travel along. The friction between the cord and the buttons is supposed to be practically infinite.

Now hang a weight *W* on to its hook at *D* and note what occurs. The cord moves from *A* toward *B*, carrying with it the buttons, which travel bodily forward without bending their

\* The electrolytic model here described is capable of considerable simplification and improvement.



supports, by reason of the slipping of their rings on *a* and *b*. This represents that no opposition electromotive force is generated in the electrolyte itself, but that it offers a little ordinary resistance to the passage of the current; also that the current is conveyed by a bodily transfer of material—that it is, in fact, a convection-current. Further, since the buttons do not slip on the cord, the length of cord between any two of them remains constant; *i. e.* there is no internal charge developed in the electrolyte. This natural length of cord between two adjacent buttons may be called the “molecular charge;” it appears to be the same (up to a simple factor) for all substances, and is what Maxwell calls *one molecule of electricity*.

Now let button No. 9 reach the electrode *Z*; it travels on, compressing the elastic supports and calling out an opposition force which may be sufficient to check the descent of *W*, or it may not. Suppose that it is not; then the button still moves on, its pressure against the knife-edge increases, until the button is cut in half and released from the cord. The tension being thus suddenly relieved, the cord makes a bound forward, the “molecular charge” of No. 9 passes over the pulleys, and No. 8 comes into contact with the electrode, to be opposed, cut, and released in like manner. When a button has been cut, it merely hangs by its rings to the rods *a* and *b*, offering a little passive obstruction because it has to be pushed forward by the others; but it presently gets pushed off the ends of the rods, and it then falls away. The cord thus progresses in a rapid succession of jerks; but as in any actual circuit the number of cords parallel to one another is very great, the average motion is continuous and not jerky.

The procession of buttons towards *Z* represents the procession of the atoms of the cation (say hydrogen) toward the cathode; when they reach it, they at first develop an opposition electromotive force at its surface; but presently getting released, they deliver up their molecular charges to it, rise through the liquid, slightly increasing its resistance, and escape as gas.

In order to diminish the polarization of the electrode, the knife-edge may be sharpened, which corresponds to making the electrode of gold or platinum instead of copper or iron; or the buttons may be softened and rendered easier to cut, which represents either a cation less electro-positive than hydrogen, or else the addition of a solvent like nitric acid. Strengthening or weakening the springs which support *Z* does not affect the electromotive force called out; but it does alter the time during which the reverse current lasts. Platinum (when not covered with “black,” as in Smee’s cell) must be

supposed to have weak springs; for the hydrogen clings tightly to it, and the reverse current, occasioned by the expansion of the compressed springs, lasts a considerable time.

I have supposed the buttons rigidly stuck to the cord; but if any electrolyte allows a trace of electricity to pass through it by ordinary conduction without equivalent electrolysis, then the cord must be allowed to slip through the buttons a little\*.

§ 14. But the model is as yet only half a one; it does not represent the oxygen proceeding up stream to the anode. To show this we require another set of buttons, ranged alongside the first set, on a cord which is driven in the reverse direction by the same weight  $W$ , and which represents a simultaneous equal opposite current of negative electricity. These buttons are also attached to rings sliding on rods  $a'$  and  $b'$ ; and these rods  $a'$  and  $b'$  must lie close to  $a$  and  $b$  respectively, so that the H buttons and the O buttons may rub against one another as they travel in opposite directions. Indeed I imagine that the rods themselves are to be infinitely smooth (and might really be dispensed with, rings and all), so that there is no real metallic resistance in an electrolyte, but only the resistance (which, however, has been found to follow Ohm's law) due to the clinging of the atoms. This clinging of the atoms is not great—it does not represent chemical combination; for, according to the theory of Clausius, it is not the chemically combined atoms which are undergoing electrolysis, but some comparatively free ones knocking about among them. Our model then only represents the *Clausian* atoms.

The electrode at which the oxygen atoms are liberated must not be supposed to impede their progress, but on the contrary to assist it, especially if made of zinc or some metal occupying a similar position in the voltaic series. It may be conceived of as attracting each button, as it comes near, against its knife-edge and cutting it off the cord; it is thus generating electromotive force in the same direction as  $W$ , and rendering any external electromotive force unnecessary for the propulsion of the cord, provided that the opposition at the cathode does not overpower it. We have here imagined a voltaic cell carrying on electrolysis and generating a current on its own account. The buttons cut off at the anode stick to it by reason of its attraction, and may be considered as combining with a portion of it. They will then tend to block up the circuit unless washed off; and so the pole gets eaten away.

Seeing that the smallest force is sufficient to make the move-

\* Faraday is said to have found this to occur in certain cases. (See Thomson on Electrolysis in the *Phil. Mag.* for December 1851, p. 431.)

ment of the free atoms preponderate in one direction, it seems worth while to try if incipient decomposition cannot be produced in a solution of double tartrate of iron, or other convenient salt, by magnetism. One might use iron electrodes, carefully depolarized, connected with a galvanometer, and arranged so that either can be magnetized at pleasure. The current produced, if too feeble to be directly observed, might be detected by using it to charge a condenser. But a more likely way would be to send a current through the solution by a very small external electromotive force, first in one direction and then in the other, and to measure it each time; in one case iron would have to be carried against magnetic force, in the other case with it (cf. Maxwell, art. 263). If the end of a magnet were cup-shaped and were filled with a *magnetolyte*, I suppose that a continuous action would go on, tending to deepen the hollow, because the middle of a magnet is magnetized more feebly than the external portions.

§ 15. If we make a heterogeneous electrolyte by taking a number of strata of different materials, such a composite structure will not be capable of internal or of residual charge as a composite dielectric is; for the buttons remain at their proper distance from one another on the cord, and this distance is the same for all the different substances by Faraday's law.

It is, however, possible for a body to possess the properties both of an electrolyte and of a dielectric; *e. g.* warm glass\*. The flow of a current through such a substance would be imitated by making the connexions between the buttons and rings in fig. 5 elastic strings instead of rigid rods. Internal charge &c. would then be possible—not, indeed, as in an ordinary dielectric, by slipping of the buttons on the cord, but by unequal slipping of the rings on the rods. It may be that conduction in dielectrics is always of this electrolytic nature, and not metallic, as was imagined in fig. 1, § 2, &c.

#### *Electrolytic Momentum.*

§ 16. The mass of oxygen carried through the liquid in one direction is eight times that of the hydrogen carried in the same time in the opposite direction; hence, so long as these materials are being accelerated, it seems as if a momentum should be imparted to the liquid as a whole, equal to the difference of momenta generated in the oxygen and hydrogen in the same time. The acceleration lasts only the inappreciably short time during which the current is assuming its constant value. Let  $v$  be the velocity imparted to a mass  $m$  of hydro-

\* See Buff (*Ann. der Chemie und Pharm.*), quoted by Maxwell, art. 271

gen in this time ; then, if  $M$  is the mass of the liquid and containing vessel, the vessel will apparently be kicked backwards with an initial velocity  $v'$ , where

$$Mv' + mv - 8mv = 0. \quad . \quad . \quad . \quad . \quad (1)$$

Now suppose the containing vessel to be a cylinder of which the electrodes form the ends, each of area  $a$ , at a distance apart equal to  $d$  ; let  $\rho$  be the density of the electrolytic liquid ; and let  $\sigma'$  be the maximum density of the electrolyzable hydrogen in the liquid—that is, the mass, per cubic centimetre of the liquid, of hydrogen actually in motion near the cathode under the influence of the current. Then, if  $h$  cubic centimetres of hydrogen gas at the density  $\sigma$  be liberated per second by the current when constant, the mass of this hydrogen will be

$$h\sigma = av\sigma'. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The *average* density of the electrolyzable hydrogen may be considered as  $\frac{1}{2}\sigma'$  ; so the whole mass of hydrogen set in motion through the liquid is

$$m = \frac{1}{2}ad\sigma' = \frac{h\sigma d}{2v}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

while the mass of liquid and containing vessel

$$M = ad\rho + \mu.$$

Hence, neglecting the mass  $\mu$  of the containing vessel,

$$v' = \frac{7mv}{M} = \frac{7h\sigma}{2a\rho}. \quad . \quad . \quad . \quad . \quad (4)$$

This velocity would be extremely small ; but it may be worth looking for. The electrolytic cell should have the plates small, because their area occurs in the denominator of (4), and it should be suspended at the end of a short counterpoised torsion-arm. Any twisting of the suspending thread might be observed with a mirror and scale. The current should not be allowed to last long enough to evolve any gas ; or the reaction of the rising gas might swamp the effect looked for. The best plan would seem to be to reverse the current at regular short periods corresponding with the previously ascertained period of oscillation of the torsion-arm. The impulses would then accumulate, and the gas would not form on the plates in any quantity.

The connexion indicated by (2) between the velocity of electricity and the mass of moving electrolytic hydrogen in a cubic centimetre of the electrolyte seems to be of interest,

if it be correct, viz.

$$v = \frac{h\sigma}{a\sigma'}.$$

This velocity  $v$  is not the rate of propagation of potential; it is the actual velocity of the electricity itself (on the hypothesis that it travels with the ions) when passing through that particular cell and liberating  $h\sigma$  grammes of hydrogen gas every second. I see no way, however, of ascertaining the unknown density  $\sigma'$ , except through a determination of  $v$  by an experiment founded on equation (4).

I have hitherto supposed water to be the electrolyte; but practically, of course, dilute sulphuric acid would be used, and it would be the group of atoms  $\text{SO}_4$  which would travel against the current, and not oxygen. This group being forty-eight times as heavy as hydrogen, we must read 47 for 7 in the above formula. It therefore seems reasonable to expect that if the method succeeded and gave measurable results, information might be obtained by means of it concerning the actual chemical action caused directly by the electric current, independent of any secondary chemical action to which the products ultimately liberated at the electrodes may be due.

Besides the sudden kick mentioned above, a *continuous* motion of the vessel seems possible, because fresh matter is continually being set in motion, while the matter already in motion is being ejected. The momentum generated per second from this cause should be equal to  $47h\sigma v$ . But then the impact of the matter liberated at the electrodes might exert force on them, which would be almost exactly equal and opposite to that exerted on the ions. Various ways, however, suggest themselves of looking for the two forces separately, or of making them assist instead of oppose each other; and it is easy to arrange so that there shall be no evolution of gas. The impact of the cathion would be advantageously observed by depositing silver on a flat electrode, varnished on one side, and hung to the end of a torsion-arm—remembering that rotation due to the earth's magnetism, and probably other disturbing causes, such as "migration of the ions," will have to be eliminated.

#### *Different Electromotors.*

§ 17. We have hitherto supposed the electromotive force to be applied to the cord in the shape of a weight hung to it. This may be said to represent roughly a battery or thermopile

\* It would be more correct to write  $\sigma' \left(1 - \frac{v}{2d}\right)$  instead of  $\sigma'$  in this and equation (2). The whole of § 16, however, is very questionable at present.

exerting a constant electromotive force. But we also, in the consideration of electrolysis, saw a little further into the real action going on in a voltaic cell ; and we shall probably, in a subsequent communication, try to look into the internal mechanism of a thermoelectric joint.

There are other ways, however, of moving the cord than by a weight. We may attach a piece of elastic to it, stretch the elastic, and fasten its free end to a nail ; the elastic would then tend to unstretch itself ; and it would pull the cord a certain distance, quickly or slowly according to the resistance of the circuit. This represents a current due to the subsidence of a given difference of potential such as would be produced in a wire by arranging it between a sphere charged with a certain quantity of electricity and the earth, or by using it to join up the coatings of a charged Leyden jar.

A third way of moving the cord would be to wind it round an axle and then to turn the axle with a winch. This would represent electromotive force generating difference of potential or doing other work at a limited rate, as in a magnetoelectric or an ordinary electric machine. To represent the latter, the axle must be thin and the handle turned slowly ; the current produced would then be very weak at the best of times, but it would be little affected by increasing the external resistance, and it would be able to break through the elastics of a dielectric, *i. e.* to produce a disruptive discharge. If slipping takes place between the cord and the driving-apparatus, it means that energy is being wasted by some means, such as friction in a machine or "local action" in a battery.

#### *Effects of rapidly reversed small Electromotive Forces.*

§ 18. Instead of pulling the cord continuously in one direction or the other, let us shake it rapidly to and fro. In a perfect dielectric no energy will be lost ; for the buttons are carried with the cord, and their elastics exert a restoring force, which tends of itself to make the cord oscillate with a succession of simple harmonic motions. But in a metallic conductor the cord simply slips through the buttons, no restoring force is called out, and the energy of the displacement is lost by friction.

These small to-and-fro motions represent the effect of waves of light falling on the body ; for, whatever the cord does or does not correspond to in nature, it certainly represents a continuous incompressible medium connecting together the particles of bodies ; and supposing light to be small and rapid oscillations of such a medium, it would be represented by small and rapid oscillations of the cord. But the linear extension of

a cord, though well enough fitted to picture a linear electric current or a stream-line, is not well adapted for the representation of the continuous medium at rest. One can, however, partly get over this by supposing that the cords are lying in all directions, and that all are connected together in some way\*.

Let us then picture to ourselves a dielectric medium as an assemblage of particles (or buttons) joined together by elastic strings and threaded by straight inextensible cords which form three mutually connected sets, each set at right angles to the other two. Imagine a set of transverse waves running down the set of cords parallel to the axis of  $z$ . The cords in the direction  $x$  and  $y$  will be shaken backwards and forwards with their buttons; but no energy will be lost, and the disturbance will pass right through the medium at a rate dependent on the  $z$  cords themselves, on the masses of the particles, and on the elasticity of their joining threads. An insulating medium is then a transparent medium, provided it is sufficiently homogeneous not to scatter the light, and provided it does not contain pigments or other foreign absorbent materials.

But now form an image of a metallic conducting medium: the cords, as before, thread particles in three cardinal directions; but the particles are now very smooth and are connected to each other, not by elastic strings, but by nearly rigid rods. Oscillations travelling down the axis of  $z$  displace, indeed, the  $x$  and  $y$  cords; but since these slide through their buttons, no restoring force is thereby called out, and the disturbance does not succeed in penetrating far into the medium before its energy is all lost in friction—converted into heat. A conducting body is necessarily an opaque body (see Maxwell, art. 798).

In a dielectric the connexion between a button and the cord is good, but the connexion between one button and another is lax. Motion of the cord is therefore readily and quickly imparted to the buttons, or *vice versa*; i. e. dielectrics are good absorbers and radiators†. Motion of a button is only slowly transmitted to other buttons; they are in general bad conductors of heat. In a metal, on the other hand, it is not easy to set the buttons swinging by means of the cord; but if a button be once set in motion, its motion is rapidly transmitted (by conduction) through the mass. There is here no *explanation*

\* Maxwell's theory is not responsible for the tendency in the model to make out that an electric current is a current of æther.

† Cf. Rankine "On the Hypothesis of Molecular Vortices," Phil. Mag. July 1851, p. 62, supposition 3. The "*atmospheres*" of his atoms appear to correspond to buttons, while their "*nuclei*" agree to some extent with the cord.

indicated of the close relation known to exist between electric conductivity and conductivity for heat. They appear to depend on two distinct things—the first on the “friction” between the button and the cord, the second on the elasticity of the rods or strings connecting the buttons together. Nevertheless it is an experimental fact that the two apparently distinct things are inverse functions of one another; not improbably they are, in substances of similar chemical properties, simply inversely proportional to one another.

§ 19. But now what happens when light falls on an electrolyte? If, indeed, the whole substance were composed, as our model would suggest, of atoms in a nearly disjointed state, it can hardly be doubted that much energy would be lost as light in effecting decomposition. But in reality these free “Clausian” atoms are very few compared with the atoms in firm chemical combination at any one instant; and all the compound molecules would be highly elastic, and would propagate the light-oscillations perfectly well, the force being by no means sufficient to pull the constituent atoms asunder in most liquids. Hence electrolytes, though conductors, may be transparent. It may possibly happen that some of the waves are to some extent destroyed by the occasional free atoms, and that the absorption of light in deep transparent liquids is partly due to this. Again, it may happen in some electrolytes that the atoms in the compound molecules themselves are so weakly associated as to be shaken asunder even by light-oscillations, especially if their periods are synchronous. This dissociation would be especially likely to manifest itself in case one or both of the constituents tended, as soon as free, to come out of the fluid in the solid state; and accordingly it is observed in a solution of sulphuretted hydrogen and in Professor Tyndall’s “actinic clouds.”

§ 20. If through a dielectric medium such as we have imagined above (§ 18) we cause an electromotive force to act in the direction of  $x$ , we must picture it to ourselves as a pulling of half the cords which lie parallel to  $x$  in the positive direction, and the other half in the negative direction (see § 8, foot-note); and the elastics being stretched by this action, an  $x$  oscillation will take place about the new position of equilibrium in a shorter period than before. Let fall in the direction of  $z$  a beam of plane-polarized light. If it were polarized so that its vibrations occurred in the plane  $xz$ , it would now travel quicker through the medium than when the electromotive force was not acting; on the other hand, if its vibrations occurred in the plane  $yz$ , it would travel apparently at its ordinary rate. (In reality its velocity would in this case



be just as much diminished as the velocity in the first case is increased, because of the relaxation of tension normal to the lines of force; but this the model does not show.) Let the real plane of polarization be  $xy$ . Then, resolving each oscillation into two equal components parallel to  $x$  and  $y$  respectively, the  $y$  component will be retarded behind the other in their passage through the medium; and when the beam emerges, it will be no longer plane, but elliptically polarized. The amount of ellipticity depends on the difference of the velocities of the two components (which depends on the square of the electromotive force), and on the thickness of strained medium through which the light has passed. If one component were retarded a quarter wave-length behind the other, the emergent light would be circularly polarized; so the actual retardation must be extremely small, as the change in the light is, even under favourable circumstances, barely perceptible. This is the phenomenon long looked for by Faraday, but first observed by Dr. Kerr\*, of Glasgow, last year.

University College, London.

*Note added October 14, 1876.*

Since the above was in print I have seen a paper of Sir William Thomson's in the Philosophical Magazine for June 1853, "On Transient Electric Currents," from which I gather that Weber had already applied his electro-dynamometer to the investigation of transient currents—in which, moreover, I find that Thomson predicted the experiment of Dr. Feddersen, mentioned above (§ 3 and footnote, p. 355), almost exactly, and calculated the value of every oscillation in a discharge. It is also shown that all the oscillations ought to occur at equal intervals of time, whatever be the resistance  $\rho$  of the discharging circuit, provided only that it remains constant. The interval is not indeed the simple harmonic

semiperiod  $\frac{\pi}{\sqrt{(n\kappa)}}$ , but it is  $\frac{A\pi}{\sqrt{(A n\kappa - \frac{1}{4}\rho^2)}}$ ,  $A$  being a constant which I do not yet understand, but which Thomson calls "the electrodynamic capacity of the discharger." I imagine, therefore that the increasing intervals actually observed were due to the resistance of the air, across which the discharge took place, increasing as the strength and heat of the successive sparks diminished.

It appears that the model agrees perfectly with Thomson's theory, except that it locates the principal cause of the action in the

\* Phil. Mag. November and December 1875. The most singular point in Dr. Kerr's discovery is his observation that bodies may be divided into two classes—those which act as if compressed along the lines of force, and those which act as if extended. I have repeated his experiments with glass, but not without failures sufficient to excite my admiration for the skill and patience involved in the discovery.

charged body instead of in the discharging wire; for, taking the model of a condenser of capacity  $C (= (n\kappa)^{-1})$ , and discharging it through a circuit of resistance  $\rho$ , the restoring force, at any instant during the discharge when the cord is displaced  $f$  and is flying back with velocity  $u$ , is

$$C^{-1}f - \rho u,$$

there being of course no time for any slip between cord and buttons. Writing this thus, with  $A$  for the total mass of all the displaced particles,

$$A \frac{d^2 f}{dt^2} + \rho \frac{df}{dt} + \frac{1}{C} f = 0,$$

and noticing that  $f = q$ , and that when  $t = 0$   $q = Q$  and  $u = 0$ , we obtain at once Thomson's fundamental equation, (5) p. 395, from which all his results follow.

In this paper I have abstained from mentioning current-induction, because I have not yet read Maxwell's second volume; but the model suggests ideas as to the nature of the process concerned in producing the extra-current &c., which I expect will turn out useful.

*XLV. Theoretical Explanations of Additional Phenomena of the Radiometer. By Professor CHALLIS, M.A., F.R.S., F.R.A.S.\**

AT the end of the "Theory of the Radiometer" which I proposed in the *Philosophical Magazine* for May 1876, I stated that I gave with reservation the explanation of the rotation of the glass globe when it is floating in water, not having then seen Mr. Crookes's communication "On the Movement of the Glass Case of a Radiometer" contained in the 'Proceedings of the Royal Society,' No. 168, p. 409. On reading that communication, I found experimental proof that the movement of the glass globe was indicative, as I had inferred from theoretical considerations, of "*friction* between the glass support and the point on which the system of vanes turns." This explanation has recently been confirmed by an experiment made by M. Jeannel (see *Phil. Mag.* for October 1876, p. 320), who observed that the rate of rotation was influenced by musical vibrations excited in the surrounding air, and reasonably attributes this effect to momentary suspensions of the friction between the pivot and its support caused by oscillations impressed on the instrument by the aerial vibrations.

The main purpose of the present communication is to take into theoretical consideration some additional experiments of

\* Communicated by the Author.

a very remarkable character, announced by Mr. Crookes in the 'Proceedings of the Royal Society,' No. 172, p. 136. I refer chiefly to the phenomena exhibited by "a small piece of pith hanging down like a pendulum at the distance of about a millimetre from the rotating vanes of the radiometer." It was noticed that "scarcely any movement of the pendulum was produced when the rotation was very rapid; but at one particular velocity the pendulum set up a considerable movement." At the suggestion of Professor Stokes, the candle which by its light produced the rotation, was placed at the distance from the radiometer for which a revolution of an arm of the fly synchronized with a vibration of the pendulum. "In this way the pendulum was kept for some time swinging through a large arc."

To account theoretically for these facts, I have, first, to direct attention to that part of the article in the May Number where it is said (p. 396) that "the incident light thus produces an abnormal state of the atoms at and near the superficies of the vane, analogous in some degree to the state of the superficial atoms of a body *electrified* by friction." Since this was written I have seen reasons (which will presently be adduced) for concluding that the light, or heat in the radiant form, incident on the vanes has the effect, after being transmuted into heat of temperature, of so changing the relative positions of the atoms in a superficial stratum as actually to induce the electric state. According to the hydrodynamical theory of electricity which I have proposed in the Philosophical Magazine for October 1860, and in 'The Principles of Physics,' pp. 544-546, the electrified state of a solid body is solely and necessarily the result of a displacement of the atoms constituting a thin superficial stratum from their normal positions. According to the same theory such disturbance of the superficial atoms is always accompanied by an interior gradation of atomic density, in consequence of which ætherial streams are generated and maintained by the action of those ætherial vibrations to which are due, under normal circumstances, the attractive and repulsive forces treated of in my communication in the September Number. It was, in fact, argued in the "Theory of the Radiometer," given in the May Number, that the state of the vanes is such as is here stated, although they were not directly said to be electrified. I shall now assume that they are in the condition of electrified bodies, and proceed to inquire what consequences follow from this supposition relatively to the new facts it was proposed to account for.

Since the light, or heat in a radiant form, incident on the vanes is converted into heat of temperature in greater degree

at its blackened surface than at the other, the two surfaces are electrified in different degrees; and, relatively to a neutral state, one is positively electrified and the other negatively electrified. Now by an experimental law of electrical action (which is also accounted for by the hydrodynamical theory), a face of the vane, whether positively or negatively electrified, as it approaches the piece of pith will *attract* it; and the oppositely electrified face of the same vane, after passing the pith, will also attract it, supposing there is no contact between the vane and the pith. Consequently the piece of pith is drawn in opposite directions in quick succession if the rotation of the vanes be very rapid; and as impression of motion takes time, it might well happen that, under these circumstances, no perceptible motion takes place. If, however, the rotation is slow, the attraction of the vane in one direction may take effect before that in the opposite direction commences, in which case an oscillatory motion of the pith ball will be produced. Clearly the oscillations will be most steady when by reason of synchronism of the time of oscillation of the pith with the time of revolution of the vane, the attraction of a given vane acts like gravity on a pendulum. If the oscillation of the pith ball be produced, as seems to be supposed, by the intervention of the action of the vanes on the residuum of air in the globe, it is wholly inexplicable that there should be no perceptible effect when the rotation of the vane is very rapid, and might be expected to cause great disturbance of the air. The facts that no oscillation is produced by a rapid rotation, and that oscillation commences after diminishing the rate of rotation, are well accounted for by the present theory; and these explanations justify at the same time the assumption of the electric state of the vanes.

In the *Institut* of July 5, p. 213, an account is given of an experiment by M. Ducretet, according to which, by throwing ether on the glass globe to produce depression of temperature, the rotation of the vanes is first stopped, and then, by continuing the cooling, is caused to take place in the opposite direction. This result is quite in accordance with the present theory. The cooling effect of the ether, according to the law of heat-exchanges, causes the vanes to radiate less heat, or even changes the heat into cold; and as the cooling, for the same reason as the heating, is in excess at the blackened surface, the rotation might thus be stopped, and the rotating force might be made to act in the contrary direction.

The movement of the vanes is affected by two causes—the presence of a residuum of air, and the friction at the pivot above spoken of. Before exhaustion of the globe, no motion

of the vanes takes place—the reason being, according to the present theory, that no electricity can pass, air of ordinary density being a non-conductor. On producing exhaustion the rotation commences and increases up to a certain point, because rarefied air is a conductor of electricity. After a high degree of exhaustion is attained, the rate of rotation diminishes, because vacuum is a non-conductor of electricity, and an approach to that condition has a retarding effect. As the exhaustion proceeds, the effect of the friction at the pivot becomes more prominent, which, however, appears from the experimental results to be, after all, a very minute quantity. A degree of exhaustion which actually stopped the vanes was not reached. I can see no *à priori* reason why the stoppage should occur with the same degree of exhaustion as that which would prevent the passage of a galvanic spark under *particular* conditions. The vanes continued to move when the degree of exhaustion was considerably greater than that at which the spark ceased to pass under the particular conditions arranged for the comparison.

On further trial I failed to verify the statement made in the May Number, that the presence of a magnet in the neighbourhood of a radiometer affected the rate of its motion. On the contrary, I proved by more careful experiments that the streams of a magnet had *no* perceptible effect on the movement of the vanes. This result might have been anticipated from the circumstance that the electric streams of the theory traverse the vanes *transversely*, so that the addition of magnetic streams (with which, according to hydrodynamics, the electric streams may coexist) produces no variation of ætherial density at the vanes, and therefore no motive force tending to give them motion. The case would be different for a set of streams symmetrical with respect to an *axis*, inasmuch as additional streams resolved transversely to the axis would, on one side of it, coincide in direction with the original streams resolved in the same direction, and on the other side be opposed to them, so that at the axis there would be a gradation of ætherial density tending to impress motion.

I think it right to explain here that the Theory of the Radiometer I have proposed is wholly founded on those *à priori* principles (stated at the beginning of the article in the September Number) which form the basis of the theories of the different physical forces which I have for a long time been engaged in discussing and verifying, and that consequently it comes into no kind of competition with any empirical theory which the experimentalist may be able to certify by the means at his command. An established empirical theory might, how-

ever, be employed to test the truth of the *à priori* theory, because, if the latter be true, it ought to be capable of giving reasons for the experimental facts on which the empirical theory is founded. The phenomena of the radiometer have attracted my attention as being peculiarly adapted to be of service in carrying on researches as to the laws of physical force. I have, in fact, been able, by applying to them the foregoing theoretical discussion, to settle an important physical question. In my Theory of Electricity I had occasion to speak of *electricity-radiants*, but had no means of determining in what order they stood relatively to heat-radiants. The tendency of the explanations given in this communication is to prove their identity, at least, as to kind, if not in degree, with heat-radiants, and to show that, like these, they are subject to the law of exchanges. On the assumption that such is their character, I propose to conclude this communication with giving a theory of the *induction* of electricity more precise than that contained in arts. 12-16 of the "Theory of Electric Force" in the Phil. Mag. for October 1860, or that in pp. 521-531 of 'The Principles of Physics.'

This theory of electrical induction rests essentially on a certain state of the atoms at and near the surface of a body (first recognized by Poisson), according to which through an extremely small thickness the density of the atoms increases from the surface towards the interior. The fulfilment of this condition is necessary for the equilibrium of the atoms so situated. It will now be supposed that the undulations of the æther corresponding to the radiants we are concerned with can traverse substances freely without undergoing transmutation. Such undulations pertain to the electrified state of a solid body, and, in conjunction with the forces of atomic repulsion, maintain by attractive action the interior gradation of density which is a necessary condition of that state. When a body in a neutral state is brought into the neighbourhood of an electrified body, those radiants of the latter that are incident upon it pass freely through it, and consequently traverse the whole of the thin superficial stratum of varying atomic density spoken of above. Now when æther in motion, whether the motion be vibratory or steady, permeates a collection of atoms varying as to the number in a given space from point to point, there is always an increment of the velocity of the æther, and therefore a decrement of its condensation, towards the parts of greater atomic density. The variation of pressure consequent upon this variation of condensation acts upon the atoms and displaces them from their normal positions; and this, as we have already argued, suffices to induce electricity. It is, how-

ever, to be considered that this action is always towards the interior of the substance, and that the induction of electricity depends on the *difference* between the disturbances at the two positions at which the radiant cuts the surface of the body, which difference arises from the variation of the intensity of the action according to the law of the inverse square. It is evident that, although the radiants may be effective in producing motion of the atoms in the superficial stratum by reason of the variation of atomic density that subsists there, they may still be of an orderw hich, like light- and heat-radiants, produce no movement of the body as a whole.

Cambridge, October 17, 1876.

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XLVI. *On certain large Crystals of Enstatite found by W. C. Brögger and H. H. Reusch at Kjørrestad near Bamle, South Norway. Memoir by W. C. BRÖGGER of Christiania and G. VOM RATH of Bonn\*.*

[Plate IV.]

THERE are few minerals which offer an equal interest, from the point of view of the progressive development of our knowledge of them, with the two magnesian silicates, enstatite,  $\text{MgSiO}_3$ , and olivine,  $\text{Mg}_2\text{SiO}_4$ . It is well known that our knowledge of the latter mineral, in proportion as it is increased, has recognized its abundance and importance. At first found almost exclusively in volcanic rocks and in small crystals, olivine has since been met with in plutonic and metamorphic rocks, the "serpentine crystals" of Snarum having been recognized as pseudomorphs after olivine. This mineral further acquires a really universal importance through its presence in meteorites.

Nearly the same may be said of enstatite. Its distribution and its importance have hitherto been less recognizable than in the case of olivine. Professor Kenngott, nearly twenty years ago, gave the name of enstatite to a mineral from Mount Zdjar in Moravia, the composition of which von Hauer determined as that of a normal silicate,  $\text{MgSiO}_3$  (*Akad. Wien*, vol. xvi. p. 162, 1855). Professor Des Cloiseaux showed by means of optical researches that the crystalline system of enstatite is rhombic, and established the difference between augite and enstatite (*Bull. Soc. Geol.* vol. xxi. p. 105). Professor Rammelsberg first proved by chemical analysis that enstatite is a constituent of meteorites, the stone of Bishopville (*Monatsber. Akad.*

\* Communicated by the Crystallometric Association. Received October 9,—Read October 27, 1876.

*Berlin*, 1861). Professor N. Story Maskelyne found this mineral in the Busti meteorite (Trans. Roy. Soc. clx. 189). Professor von Lang showed, in an admirable investigation, how rich were the combinations of the enstatite of the Breitenbach meteorite. Nearly at the same time vom Rath determined the crystals of hypersthene of Laach. The specimens from both these sources, cosmical and terrestrial, have absolutely the same angles. Implanted crystals of enstatite presenting freely formed crystalline faces had hitherto been unknown in plutonic rocks. It might be thought that if they could be found they might astonish us by their gigantic size, like the olivines of Snarum; and in this expectation we are not disappointed.

In the apatite mine of Kjørrestad between Kragerø and Langesund, in the autumn of the year 1874, enstatite was discovered in crystals of a size reached only by very few minerals. The locality where the great enstatite crystals have been found is one of those numerous apatite veins of Southern Norway. The main rock of this part of the coast is mica and amphibole slate, in which the apatite veins are included. Their predominating mineral is amphibole.

In the vicinity of these normal veins there is to be found at the Hankedalsvånd, not far from Vestre Kjørrestad, an isolated deposit, as a thick lode constituted principally of large crystals of enstatite and enormous masses of rutile. There was not much apatite, but some greenish-white mica and talc; and for this reason apatite was obtained only for a short time. The enstatite crystals, from 0.3 to 0.4 metre in size, had been thrown away unheeded, till they were discovered by Brögger and Reusch in their researches on the apatite beds (*Zeitschr. d. deutsch. geol. Gesellsch.* vol. xxvii. p. 646, 1875). As the mine of Vestre Kjørrestad had been already abandoned, the investigations of the discoverers were confined to the matter thrown out.

The enstatite crystals, occurring in more or less elongated columnar forms always broken at one end, were no doubt originally implanted on the wall of the lode. The space between the gigantic enstatite crystals was filled with silver-white or light-green talc. Also in the interior, and principally in the decomposed crust of the enstatite, we see minute scales of talc, lying in planes parallel to the prismatic faces, or oftener to the brachypinakoid, formed evidently by a metamorphosis of the enstatite.

The new crystals excite our attention at first by their size. Several crystals attain a magnitude of 20 centims. in length and in breadth. One of the two largest specimens measures



38 centims. in length, 26 in breadth, 13 in thickness; the other one, notwithstanding its being broken at both ends, is still 40 centims. long.

The crystals show a predominating vertical rhombic prism, whose edges are nearly rectangular. The makropinakoid is much developed, whilst the brachypinakoid is only small. An evident rhombic symmetry is seldom to be recognized on the summits of the crystals; it offers, on the contrary, a pseudomonoclinic appearance. A very characteristic feature of the crystals is the great number of often repeated faces, and with small inclinations which form a shallow vaulted summit. We shall return to the study of the crystalline form after a brief sketch of the physical and chemical properties. The surface of the crystals offers a steatitic appearance; the faces are dull. This steatitic crust is only thin, reaching at the utmost 10 millims. into the interior. There are two perfect cleavages, parallel to the prismatic faces; the third, parallel to the brachypinakoid, is imperfect. The lustre of the first two is pearly, whilst that of the imperfect cleavage is only glistening. Numerous undulated fissures seen on the cleavage-faces are connected with the metamorphosis into steatite. It is very interesting by means of the microscope to observe the gradual change of enstatite into the hydrous combination. The specific gravity of our enstatite is 3.153. The composition is as follows:—

SiO <sub>2</sub>	= 58.00	O	= 30.93
Al <sub>2</sub> O <sub>3</sub>	= 1.35	„	0.63
FeO	= 3.16	„	0.70
MgO	= 36.91	„	14.76
H <sub>2</sub> O	= 0.80		
	<hr/>		
	100.22		

These numbers resemble closely the composition of the Moravian specimen from the Zdjár Mountain, and those of the cosmical enstatites from Bishopville and Busti.

We also analyzed the steatitic crust, whose specific gravity = 2.867.

SiO <sub>2</sub>	= 57.62
Al <sub>2</sub> O <sub>3</sub>	= 1.48
FeO	= 1.96
CaO	= 0.12
MgO	= 34.72
H <sub>2</sub> O	= 4.38
	<hr/>
	100.28

So that the only change is the introduction of water.

Considerable difficulties present themselves in the optical investigation of the Kjørrestad enstatite, arising from the perfect cleavages and the imperfect transparency. Professor Des Cloiseaux had the kindness to undertake the research, and determined the crystalline system from the optical character to be rhombic. It was done at a time when we were still in doubt concerning the form, and especially as we had not yet reduced the forms of the Kjørrestad crystals to those of bron-zite and enstatite.

According to Professor Des Cloiseaux, the optical axes lie in the brachypinakoid. The acute positive bisectrix is parallel to the edge of the vertical prism. A plate cut normally to this edge gave (in oil):—

$$\begin{array}{rcl} \text{Right hyperbola to the normal} & 37 & 30 \\ \text{Left} & \text{''} & \text{''} & 41 & 0 \\ & & & \hline & 2 H_{a,r} = & 78 & 30 \end{array}$$

Most of the crystals, as already mentioned, show a remarkable deformity, which consists in a dislocation or turning (round the makrodiagonal as axis) of all the faces forming the summit. The brachydiagonal becomes, to all appearance, klinoaxis. Whilst the crystals show, in consequence of this deformity, an asymmetric form on both sides of the macrodiagonal plane, the brachydiagonal plane becomes the face of symmetry. It is difficult to believe them of a rhombic character on looking at only a few of these crystals. But examining more of them, we get convinced, even without measuring, that the degree of the dislocation is different in each crystal, and that the whole phenomenon is to be reduced to perturbations.

But all the crystals are not deformed: some of them are of pure rhomboidal character; and these allow a crystallographic identification with the enstatite of Breitenbach (following von Lang, *Sitzungsber. Ak. Wien*, vol. lix. 2. Abth. Aprilheft, Jahrg. 1869), and with the hypersthene from the Rocher du Capucin in Montdore and from the lake of Laach (Des Cloiseaux, *Manuel de Minéralogie*, vol. ii. pp. xiv–xviii; vom Rath, *Pogg. Ann.* vol. cxxxviii. p. 529, and vol. clii. p. 29).

In order to be able to compare the crystals of the three above-mentioned localities, we have drawn in the figures 1, 1a, 2, 2a, 3, 3a the crystals of Breitenbach (meteoric), Laach, and Rocher du Capucin.

We choose as the primitive form the pyramid measuring in the

	On the enstatite of Breitenbach.	On the hypersthene of Laach.
Makrodiagonal edge	$= 125^{\circ} 52'$	$125^{\circ} 58\frac{1}{2}'$
Brachydiagonal edge	$= 127^{\circ} 36'$	$127^{\circ} 38\frac{1}{2}'$
Lateral edge . .	$= 78^{\circ} 42'$	$78^{\circ} 34\frac{1}{2}'$

These angles give the following axis :—

$$a \text{ (brachydiagonal)} : b \text{ (makrodiagonal)} : c \text{ (vertical)} \\ = 0.97016 : 1 : 0.57097$$

for the Breitenbach meteoric enstatite, or

$$= 0.971326 : 1 : 0.57000$$

for the hypersthene of Laach.

The faces represented in the figures receive the following formula if we refer to the preceding axes :—

	Des Cloiseaux.	von Lang.
$o = (a : b : c), P$ .....	$b^{\frac{1}{2}}$	1 1 2
$i = (\frac{1}{2}a : b : c), 2\bar{P}2$ .....	$a_3$	1 2 2
$e = (a : 2b : c), P2$ .....	$n$	1 2 4
$u = (a : \frac{2}{3}b : c), \frac{3}{2}\check{P}\frac{3}{2}$ .....	$x$	
$x = (\frac{1}{2}a : \frac{1}{2}b : c), 2P$ .....		1 1 1
$m = (a : b : \infty c), \infty P$ .....	$m$	1 1 0
$n = (2a : b : \infty c), \infty \check{P}2$ .....		1 2 0
$z = (a : 2b : \infty c), \infty \bar{P}2$		
$h = (\infty a : 4b : c), \frac{1}{4}\check{P}\infty$ .....	$e^4$	
$k = (\infty a : 2b : c), \frac{1}{2}\check{P}\infty$ .....	$e^2$	1 0 4
$a = (a : \infty b : \infty c), \infty \bar{P}\infty$ .....	$h^1$	0 1 0
$b = (\infty a : b : \infty c), \infty \check{P}\infty$ .....	$g^1$	1 0 0
$c = (\infty a : \infty b : c), 0P$ .....	$p$	0 0 1

There was only one angle on the crystals of Kjørrestad which could be measured with the reflection-goniometer—that is to say, the angle of the two cleavage-faces,  $= 91^{\circ} 25' - 91^{\circ} 40'$ . Von Lang determined this angle on the enstatite of Breitenbach  $= 91^{\circ} 44'$ . On the hypersthene of Laach I found this angle  $= 91^{\circ} 40'$ ; whilst Des Cloiseaux gives the angle  $= 91^{\circ} 32\frac{1}{2}'$  for the hypersthene of Capucin, as the mean of several measures. All the other angles of the Kjørrestad crystals can only be measured with the contact-goniometer, and even that only approximately, in consequence of the striation, the arching, oscillation, and repetition of the faces, sometimes already decomposed.

One of the largest crystals in the Museum of Christiania

(measuring along the axis  $a$  12 centims., along the axis  $b$  20 centims.) shows undoubted rhombic symmetry. We have determined the following faces:—

$$\begin{aligned}
 e &= (a : 2b : c), & P2, \\
 \epsilon &= (a : \frac{3}{4}b : c), & \frac{4}{3}\bar{P}\frac{4}{3}, \\
 \psi &= (\infty a : 6b : c), & \frac{1}{6}\bar{P}\infty, \\
 \gamma &= (\infty a : \frac{7}{2}b : c), & \frac{2}{7}\bar{P}\infty, \\
 \kappa &= (\infty a : 2b : c), & \frac{1}{2}\bar{P}\infty, \\
 q &= (\infty a : \frac{3}{2}b : c), & \frac{2}{3}\bar{P}\infty, \\
 m &= (a : b : \infty c), & \infty P, \\
 a &= (a : \infty b : \infty c) & \infty \bar{P}\infty, \\
 b &= (\infty a : b : \infty c), & \infty \bar{P}\infty, \\
 c &= (\infty a : \infty b : c), & 0P.
 \end{aligned}$$

The faces denoted by Latin letters are already known on the enstatite from Breitenbach and on the hypersthènes from Laach and Capucin. The faces denoted by Greek letters are new. The fig. 4 represents the crystal in question with all deformities. The following angles were measured with the contact-goniometer. It will be seen that they compare satisfactorily with the values calculated from the elements of the Breitenbach enstatite.

	Measured.	Calculated.
$m : m'$ (upon $b$ ) =	$88^{\circ} 0'$	$88^{\circ} 16'$
$k : b$	$= 105 50$	$105 56$
$q : b$	$= 111 0$	$110 50\frac{1}{2}$
$\gamma : b$	$= 99 30$	$99 16$
$\psi : b$	$= 95 30$	$95 26$
$\epsilon : k$	$= 150 30$	$147 31$
$\epsilon : b$	$= 122 30$	$123 16$
$e : b$	$= 105 0$	$103 49$
$e : a$	$= 119 30$	$119 31$
$e : \gamma$	$= 151 45$	$149 49$
$e : \psi$	$= 150 0$	$148 51$
$e : k$	$= 152 0$	$150 29\frac{2}{3}$

Only two of these measurements ( $\epsilon : k$  and  $e : \gamma$ ) show a considerable divergence from the calculated angles. It is to be explained by the difficulty and uncertainty involved in measuring faces perturbed by oscillation and vaulting.

Also it is to be noticed that the crystal was measured at Christiania, and that the faces denoted by Greek letters were determined at Bonn. Having in view the imperfections of the faces, it would not have been possible to obtain measurements more nearly approaching to the calculated angles.

Fig. 5 represents a crystal of the Museum of Bonn: 5 is a portrait of the crystal; 5*a* and 5*b* are more ideal representations. We have the following combination:—

$$\begin{aligned}\tau &= (\frac{3}{2}a : \frac{3}{2}b : c), \frac{2}{3}P, \\ k &= (\infty a : 2b : c), \frac{1}{2}\bar{P} \infty, \\ q &= (\infty a : \frac{3}{2}b : c), \frac{2}{3}\bar{P} \infty, \\ \psi &= (\infty a : 6b : c), \frac{1}{6}\bar{P} \infty, \\ m &= (a : b : \infty c), \infty P, \\ b &= (\infty a : b : \infty c), \infty \bar{P} \infty.\end{aligned}$$

	Measured.	Calculated.
$m : m' =$	$88^{\circ}$	$88^{\circ} 16'$
$k : b =$	$106$	$105^{\circ} 56'$
$q : b =$	$110^{\circ} 30'$	$110^{\circ} 50\frac{1}{2}'$
$\psi : k =$	$169$	$169^{\circ} 30'$
$\tau : q =$		$159^{\circ} 51\frac{2}{3}'$
$\tau : m =$	$120$	$118^{\circ} 40'$

We remark already a certain asymmetry in this crystal, as is to be seen in the following measurements:—

$$\left. \begin{aligned}k : m &= 101^{\circ} 57' * \\ k : m' &= 99^{\circ} 24' \dagger\end{aligned} \right\} \begin{array}{l} \text{Calculated.} \\ 101^{\circ} 1' \end{array}$$

The pyramid  $\tau$ , not yet observed in enstatite and hypersthene, has been determined by the horizontal edges formed with the prism  $m$  as well as through the parallelism of the edges  $\tau : q : \tau'$ . The dimensions of that crystal are—altitude 7 centims., breadth 8 centims., thickness 9 centims. The crystal is broken parallel to two prismatic faces.

Fig. 6 is very much like the preceding :

	Measured.	Calculated.
$m : m' =$	$88^{\circ}$	
$\tau : m =$	$118$	$118^{\circ} 40'$
$\tau : a =$	$109\frac{3}{4}$	$110^{\circ} 8\frac{1}{3}'$

\* Mean of ten measurements between  $101\frac{3}{4}^{\circ}$  and  $102^{\circ}$ .

† Mean of ten measurements between  $99\frac{1}{4}^{\circ}$  and  $99\frac{1}{2}^{\circ}$ .



both angles. The real symbol is also to be found for the disturbed face  $\sigma$ ; reduced to the system of the enstatite, it has the symbol  $(a : \frac{3}{2}b : c)$ ,  $\bar{P}\frac{3}{2}$ . For the inclination of this face we have

$$m' : \sigma = 124^{\circ} 16' ; \chi : \sigma = 151^{\circ} 11'.$$

We must compare these angles with the above in order to recognize the degree of perturbation. There are yet several other faces on the enstatite crystals from Kjørrestad, the symbols of which, in consequence of their imperfections, cannot be definitely determined. Such faces are not indicated by a letter in our figure. This richness of faces recalls to mind the meteoric enstatite of the Breitenbach iron, which were determined by von Lang's sagacity. The cause of the remarkable monoclinic formation of the Norwegian enstatite is quite hidden. The first thought may be that it is the result of pressure. But that explanation must be surrendered if we observe that the monoclinic dislocation is only to be seen in the faces of the summit, and never in the vertical faces. So this anomaly seems to have its origin in crystallonomic causes. The exhausted apatite-mine of Kjørrestad is hitherto the only known locality of these enstatite giants. No doubt implanted enstatite crystals will be found in other plutonic localities. If we shall there be able to observe the crystals not merely among the materials thrown away, but also in their natural position *in situ*, we may possibly find a solution for the dislocation of the terminating faces, which is at present an enigma.

Bonn, October 4, 1876.

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XLVII. *The Spectra of Indium.* By ARTHUR W. CLAYDEN, B.A., and CHARLES T. HEYCOCK\*.

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SOME time ago, while working at a course of lectures on spectrum-analysis, given by the Professor of Chemistry, we noticed that the spectrum of indium, as obtained by passing the spark from an induction-coil between points of the metal, differed from that given in the various books on the subject. Instead of the three lines expected, we found sixteen.

During the past summer we have investigated the matter, with the following results.

When the spark is used to decompose and volatilize the chloride of the metal, the spectrum ordinarily given in the books, namely two lines in the indigo and one in the violet, is seen. Thalén gives the following measurements for these—4532, 4509, and 4101; but we are inclined to think 4510 is nearer the truth than 4509.

\* Communicated by the Authors.

On passing the spark between the metallic points another spectrum is seen, consisting of sixteen lines spread over the range of the luminous field: the two more-refrangible of the lines shown by the chloride appear in company with fourteen of lower refrangibility, while the line for which 4532 is given is entirely absent. The highest line of all (4101) also suffers a diminution of intensity.

On measuring the new lines we find the following numbers, giving in the first column the readings on our own scale, and in the second approximately those on the scale in Dr. Watts's 'Index of Spectra.' The wave-lengths are, of course, given in tenth-metres; and in the column headed Character we have followed the notation of Huggins, so that *n* = nebulous, *sn* = slightly nebulous, and *s* = sharply defined.

Scale.	Scale (Watts).	Wave- length.	Intensity.	Character.
0.10	28.5	6906	6	<i>s</i>
7.55	42.3	6193	10	<i>s</i>
8.65	44.3	6114	2	<i>n</i>
8.90	44.7	6095	8	<i>sn</i>
11.45	49.3	5922	4	<i>n</i>
11.70	49.7	5905	4	<i>n</i>
12.35	50.9	5862	2	<i>n</i>
13.00	52.2	5820	8	<i>sn</i>
14.65	54.8	5722	4	<i>sn</i>
16.05	57.3	5644	8	<i>sn</i>
24.40	71.5	5250	10	<i>sn</i>
42.35	100.7	4680	8	a nd
43.40	102.1	4656	8	a nd
44.15	103.3	4638	8	band
50.05	111.8	4510	10	<i>s</i>
74.70	150.9	4101	8	<i>s</i>

The first line, for which 6906 is given, is very remarkable, potassium, strontium, and antimony being the only elements in the spectra of which any one has observed less refrangible lines. The line 5250 is one of the most conspicuous, and is of a bright green colour. The three which we have called bands are rather broad lines, sharply defined towards the violet and shading off towards the red. Their colour is bright blue.

On experimenting with the nitrate we obtained a curious result. The same three lines that are shown by the chloride appeared; but faint indications of the presence of at least some of the brighter lines of the metallic spectrum occasionally flashed into sight, though all our efforts to bring them out with any satisfactory clearness completely failed. We varied the intensity of the spark considerably, but found the same result in every case.



We may as well state that we have used two sets of metallic points—one lent to us by the Professor of Chemistry, the other cut from a piece of indium which we purchased from Messrs. Hopkin and Williams of Hatton Garden. The spectroscope we used is a powerful instrument with four prisms.

Cavendish Laboratory, Cambridge.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 315.]

March 30, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following papers were read:—

“An Experiment on Electro-magnetic Rotation.” By W. Spottiswoode, M.A., Treas. and V.P.R.S.

The phenomena of the rotation of movable conductors, carrying currents, about lines of magnetic force are well known. One form of experiment, commonly called the rotating spark, presents, beside the actual rotation, some peculiar features which do not appear to have been noticed in detail. The instrumental arrangements consist of a partially exhausted chamber with a platinum point for one terminal, a ring for the other, and the intervening air or other gas for the movable conductor. The chamber is made in the form of a double cylinder, so that a magnet inserted through the ring may reach nearly to the point. The discharge then passes between the point and the ring, and revolves about the magnet according to Ampère's law.

But beside the rotation, and even when, through weakening of the magnet, rotation does not actually take place, the spark, when carefully observed, is seen to assume a spiral form; and the spiral is right-handed or left-handed according to both the direction of the current and the magnetic polarity. This effect is particularly noticeable if the magnetic pole be inserted only a short distance beyond the ring. The discharge is then seen to spread itself out sheetwise on the ring in the direction in which rotation would take place. The edge of the sheet is in the form of a helix.

The object of the following observations is to bring out the character of this phenomenon by making it a principal instead of a secondary feature of the experiment.

The arrangement here described consisted in using the poles of an electromagnet as the terminals of a discharge from an induction-coil, and in observing the effect on the form of the discharge caused by exciting the electromagnet. For this purpose the movable poles were insulated from the main body of the magnet by interposing a sheet of ebonite thick enough to prevent the passage of the discharge, but not thicker, in order as little as pos-

sible to diminish the strength of the magnetic poles. The discharge was then effected either in the open air or in a closed chamber. The latter was constructed of a short cylinder of glass, say 3 inches in length and 2 in diameter, having conical ends pointed inwards, so as to receive the poles of the magnet. The chamber was also furnished with a pipe and stopcock for the purpose of exhaustion.

The discharge from an induction-coil taken in air or other gas at atmospheric pressure consists, as is well known, primarily of the spark proper or bright line, irregular in form and instantaneous in duration. But beside this, when the primary wire is thick and the battery-current strong, the spark is enveloped in a bright cloud, or rather flame, which is capable of being thrown on one side, although not entirely detached from the spark, by a current of air. This, when examined in a revolving mirror, is found to be subsequent in time to the spark proper, and may be considered to be due to the gas in the neighbourhood of the spark becoming sufficiently heated to conduct part of the discharge, and to the consequent combustion of any extraneous matter floating in the medium. Such a view is supported by the fact that the colour of this flame depends partly upon the nature of the gas in which the discharge takes place, and partly upon that of any volatilizable matter which may be introduced near the poles.

The exciting of the magnet produces upon the spark proper no appreciable effect; but as soon as the flame is submitted to its action it is spread out into a sheet, which arranges itself in a helioid right-handed or left-handed according to the direction of the current and of the magnetic polarity in obedience to Ampère's law.

Effects substantially the same are produced whether the discharge be taken in gas at atmospheric or at a less pressure. But in the former case the helix has a lower, in the latter a steeper gradient; that is to say, in the former case it presents a greater, in the latter a less number of turns for a given interval between the poles.

But for producing the best effects, both of the rotating spark and also of the spirals, there is a limit beyond which the exhaustion should not be carried. At a pressure low enough to produce stratification, or even short of it, the whole chamber is filled with the discharge, and all traces of rotation and of spirals are obliterated. The stage best suited to the purpose is that in which the discharge has thickened in diameter, and where the spark proper has been replaced by a suffused light of the thickness, say, of a quill. If the negative terminal be a surface (say the naked surface of the soft iron pole of the electromagnet) instead of a point, the sheet does not become contracted at the negative end, but remains spread out and cuts the iron pole in a line radiating from the point.

Various gases were tried—atmospheric air, carbonic acid, ether, chloroform, coal-gas, hydrogen. Of these the first two succeeded best. With air the illumination of the flame-sheet was rather

greater; but with carbonic acid greater steadiness of position was obtained. With both ether and chloroform occasional flashes, brilliantly illuminated, were seen; but some chemical action appeared to take place militating against the steady development of the flame-sheet. With coal-gas there was an inconvenient deposit of carbon upon the sides of the chamber. With hydrogen the cloud was not sufficiently developed.

The success which attended the experiment with air may possibly be partly due, as suggested above, to the combustion of the extraneous matter floating therein; and in fact the brilliancy and extent of the sheet may be increased by attaching a piece of metallic sodium to the negative terminal, or by causing a stream of any of the chlorides in powder, *e. g.* of strontium, lithium, &c., to flow across the field of action.

When a piece of sodium (or better still of soda) is attached to one of the terminals, two effects may be noticed. When that terminal is negative the whole of the flame is bright yellow, showing that the sodium is not only detached but even carried across the field and deposited on the positive terminal. When, however, the terminal to which the sodium is attached is positive, it is found that the flame, when observed through a red glass, appears yellow to a certain distance from that terminal, but red beyond, and also that the pitch of the helix is less near the positive than near the negative terminal. These effects may be attributed to the presence of metallic vapour evolved by the heat at the positive terminal, but not carried across the field as when the terminal in question is negative.

The following explanation of the phenomenon is due to Prof. Stokes, from whose correspondence it is substantially taken. The mathematical solution, although only roughly approximate, is perhaps still quite sufficient to give the general character of the experimental results.

The magnetic field will be supposed uniform, and the lines of force parallel straight lines from pole to pole. The path of the current when undisturbed is also a straight line from pole to pole. In such a condition of things, every thing being symmetrical, no rotation would take place. But if through any local circumstance, as in the experiment in air, or through heating of the chamber as in the exhausted tube, or otherwise, the path of the current be distorted and displaced, then each element will be subject to the action of two forces. To estimate these, let  $ds$  be an element of the path, with rectangular components  $dx$ ,  $dy$ ,  $dz$ ,  $C$  the strength of the current, and  $R$  the magnetic force with components  $X$ ,  $Y$ ,  $Z$ , which in the first instance will be treated generally. Then one force will be that tending to impel the current in the direction of the axes respectively, and may be expressed by

$$C(Ydz - Zdy) : ds, \quad C(Zdx - Xdz) : ds, \quad C(Xdy - Ydx) : ds.$$

Besides this, there will be the tendency of the current to follow the shortest path so as to diminish the resistance. Representing

this as a tension  $\tau$ , the components at one end of  $ds$  will be  
 $-\tau dx : ds, -\tau dy : ds, -\tau dz : ds,$   
 and those at the other

$$(\tau dx : ds) + d(\tau dx : ds) \dots,$$

the algebraical sums of which are

$$d(\tau x d : ds), d(\tau dy : ds), d(\tau dz : ds);$$

and the equations of equilibrium then become

$$C(Ydz - Zdy) + d(\tau dx : ds) = 0, \dots \dots \dots (1)$$

$$C(Zdx - Xdz) + d(\tau dy : ds) = 0, \dots \dots \dots (2)$$

$$C(Xdy - Ydx) + d(\tau dz : ds) = 0; \dots \dots \dots (3)$$

taking  $s$  as the independent variable and multiplying by  $dx : ds, dy : ds, dz : ds$  respectively, and adding, we obtain  $d\tau = 0$ , or  $\tau = \text{constant}$ . Again, multiplying by  $X, Y, Z$  and adding we obtain

$$Xd^2x : ds^2 + Yd^2y : ds^2 + Zd^2z : ds^2 = 0, \dots \dots \dots (4)$$

which expresses that the absolute normal (or normal in the osculating plane) is perpendicular to the resultant magnetic force.

In the case of a uniform tint,  $X, Y, Z$  will be constant. Integrating (4) and putting  $i$  for the angle between the tangent and the lines of magnetic force, we find

$$Xdx + Ydy + Zdz = Rds \cos i,$$

so that the tangent line is inclined at a constant angle to the line joining the poles.

Again, the following combinations, (2)  $dz - (3)dy = 0$ , (3)  $dx - (1)dz = 0$ , (1)  $dy - (2)dx = 0$  give

$$Cdx(Xdx + \dots) - CXds^2 + \tau \left( \frac{dz}{ds} \frac{d^2y}{ds^2} - \frac{dy}{ds} \frac{d^2z}{ds^2} \right) ds^2 = 0, \&c.,$$

or

$$C(R \cos i dx - Xds) + \tau \left( \frac{dz}{ds} \frac{d^2y}{ds^2} - \frac{dy}{ds} \frac{d^2z}{ds^2} \right) ds = 0, \&c.$$

Transposing, squaring, and adding, and putting  $\rho$  for the radius of curvature, we obtain

$$C^2 R^2 \sin^2 i = \tau^2 : \rho^2, \text{ or } \rho = \tau : CR \sin i,$$

which is constant. The curve is therefore a helix. Also the radius of curvature of the projection of the curve on a plane perpendicular to the axis will be  $\rho \sin^2 i$ , viz.  $= \tau \sin i : CR$ .

“The value of  $\tau$  depends doubtless on the nature and pressure of the gas, and perhaps also on the current; but it must be the same for equal values of  $C$  of opposite signs. Hence the handedness of the helix will be reversed by reversing either the current or the magnetic polarity. If the left-hand magnetic pole be north (*i. e.* austral, or north-pointing), and the left-hand terminal positive, the helix will be right-handed.”

The general nature of the phenomenon may therefore now be described as follows:—“First we have the bright spark of no sensible duration which strikes nearly in a straight line between the terminals. This opens a path for a continuous discharge, which

being nearly in a condition of equilibrium, though an unstable one, remains a short time without much change of place. Then it moves rapidly to its position of equilibrium, the surface which is its locus forming the sheet. Then it remains in its position of equilibrium during the greater part of the discharge, approaching the axis again as the discharge falls, so that its equilibrium position is not so far from the axis. Thus we see two bright curves corresponding to the two positions of approximate rest united by a less bright sheet, the first curve being nearly a straight line, and the second nearly a helix traced on a cylinder of which the former line is a generating line.

"It was noticed that the sheet projected a little beyond the helix. This may be explained by considering that at first the discharge is more powerful than can be maintained, so that the curve reaches a little beyond the distance that can be maintained."

The appearance of the discharge when viewed in a revolving mirror (except the projection beyond the sheet, the illumination of which was too feeble to be observed) confirmed the above remarks.

"On the Movement of the Glass Case of a Radiometer." By William Crookes, F.R.S. &c.

During the discussion which followed the reading of Prof. Reynolds's and Dr. Schuster's papers at the last meeting of the Royal Society I mentioned an experiment bearing on the observations of Dr. Schuster. I have since tried this in a modified form; and as the results are very decided and appear calculated to throw light on many disputed points in the theory of these obscure actions, I venture to bring a description of the experiment, and to show the apparatus at work, before the Society.

I made use of a radiometer described in a paper communicated to the Society in January last. I quote the description from paragraph 184. "A large radiometer in a 4-inch bulb was made with ten arms, eight of them being of brass and the other two being a long watch-spring magnet. The disks were of pith, blackened on one side. The power of the earth on the magnet is too great to allow the arms to be set in rotation unless a candle is brought near; but once started it will continue to revolve with the light some distance off."

This radiometer was floated in a vessel of water; and four candles were placed round it so as to set the arms in rotation. A mark was put on the glass envelope so as to enable a slight movement of rotation to be seen. The envelope turned very slowly a few degrees in one direction, then stopped and turned a few degrees the opposite way; finally it took up a uniform but excessively slow movement in the direction of the arms, but so slow that more than an hour would be occupied in one revolution.

A powerful magnet was now brought near the moving arms. They immediately stopped, and at the same time the glass envelope

commenced to revolve in the opposite direction to that in which the arms had been revolving. The movement kept up as long as the candles were burning, and the speed was one revolution in two minutes.

The magnet was removed, the arms obeyed the force of radiation from the candles and revolved rapidly, whilst the glass envelope quickly came to rest and then rotated very slowly the same way as the arms went.

The candles were blown out; and as soon as the whole instrument had come to rest a bar-magnet was moved alternately from one side to the other of the radiometer, so as to cause the vanes to rotate as if they had been under the influence of a candle. The glass envelope moved with some rapidity (about one revolution in three minutes) in the direction the arms were moving. On reversing the direction of movement of the arms the glass envelope changed direction also.

These experiments show that the internal friction, either of the steel point on the glass socket, of the vanes against the residual air, or of both these causes combined, is considerable. Moving the vanes round by the exterior magnet carries the whole envelope round in opposition to the friction of the water against the glass.

As there is much discussion at present respecting the cause of these movements, and as some misunderstanding seems to prevail as to my own views on the theory of the repulsion resulting from radiation, I wish to take this opportunity of removing the impression that I hold opinions which are in antagonism to some strongly urged explanations of these actions. I have on five or six occasions specially stated that I wish to keep free from theories. During my four years' work on this subject I have accumulated a large fund of experimental observations; and these often enable me to see difficulties which could not be expected to occur to an investigator who has had but a limited experience with the working of one or two instruments.

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GEOLOGICAL SOCIETY.

[Continued from p. 316.]

June 21, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

3. "Recent Glacial and Aqueous Action in Canada and the Drift-Uplands of the Province of Ontario." By the Rev. Wm. Bleasdel. Communicated by the President.

The author stated that an enormous amount of glacial action takes place every winter in Canada, especially on the river St. Lawrence and its large lakes. "Anchor ice" and "pack ice" are drifted down the stream, abrading and carrying off piece-meal the various small islands studded along its course. A "shove," as it is termed, often takes place on the breaking up of the ice; the broken blocks

of ice are piled up and jammed together, damming up the river; and destructive floods ensue when the icy barrier gives way. Several examples of these phenomena which came under his notice are recorded by the author.

At Presqu'isle Point, Lake Ontario, "banks" of ice are formed by the dashing up of the water. These banks exercise an abrading influence on the shore. In the winter of 1874-75 these ice "banks" were greater than ever known previously, extending 300 feet out into the lake, whilst some of the "hummocks" of ice were 20 feet high. Some sixteen years ago a portion of the shore 40 feet wide was laid bare in one winter. The rocks thus exposed were finely polished, grooved, and scratched. These marks have now been obliterated by the very agent that formerly produced them. The author also gives several instances of the formation and cutting through of sandbanks and headlands. The cutting back of the coast-line of the lake is proceeding at the rate of from 30 to 40 feet in twenty years.

Fringing the basin of Lake Ontario are terraced highlands of loose gravel, with here and there apparent stratification of a partial character, called *Artemisia-gravel* by the Canadian Survey. A peculiar feature of these gravel ridges is the presence in them of numerous small lakes. The author records the bursting of the natural dam of one of these lakes, and mentions the occurrence of a peculiar hollow, probably the basin of a lake that burst in a similar way in former times.

Freshwater-shell deposits occur here and there; further down the river a brackish-water shell is found in them; and they are replaced lower down by marine deposits. The author quotes various opinions relative to the formation of these terraces and shell-deposits by supposing the former existence of a barrier of ice or rock lower down the river, and concludes that by these phenomena noteworthy alterations are year by year being made on the surface of the land.

4. "The Glacial Climate and the Polar Ice-cap." By Joseph John Murphy, Esq., F.G.S.

The author agrees with Mr. Croll in thinking that a glacial epoch must be one of maximum eccentricity of the earth's orbit, and that the northern and southern hemispheres during such an epoch must be glaciated alternately; but he maintains, in opposition to that writer, that the glaciated hemisphere must have its *summer* in aphe-  
lion. He intends this paper to be a reply to Mr. Croll's objections to this theory as put forth in his work on *Climate and Time*. The author holds that some of Mr. Croll's conclusions are erroneous, and concludes with some remarks on Mr. Tylor's paper read before the Society in April 1875.

12. "On the Mechanism of Production of Volcanic Dykes, and on those of Monte Somma." By R. Mallet, Esq., F.R.S., F.G.S.

The author stated that in 1864 he made a careful trigonometrical survey of the escarpment of Monte Somma, especially with reference to the numerous dykes by which the rocks composing it are intersected. He described in detail the phenomena of direction of the

dykes, especially as regards the axis of the cone of Vesuvius ; to this direction he gives the name of *orientation*. Of twenty-seven dykes ten presented an approximately vertical line, whilst all the rest had a sensible dip or "hade." The dykes are in no cases intersected by coherent beds of lava ; but in one instance the top of a dyke was stopped by such a bed. Many of the dykes bifurcated or branched ; and frequently two dykes intersected each other at considerable angles. These and other circumstances prove that the dykes were produced at different and successive ages. Many of them were fractured and displaced in consequence of movements of the mass of rock traversed by them ; and these dislocations are regarded by the author as indicating the vast extent and force of the internal movements, due principally to gravity, which are constantly taking place in the mass of volcanic cones. These movements greatly influence the position of the dykes, and render it difficult to ascertain that which they originally occupied. The dykes thin out at various heights ; and their superior and northern terminations were found not to reach the existing surface, notwithstanding the amount of denudation that has taken place ; hence the author concludes that they never reached the surface of Somma when it was the wall of an active volcano. The author further indicated a process by which beds or plates of lava descending the slopes of a volcano may change their direction, and, becoming imbedded in the detritus accompanying or following them, may, to a greater or less extent, simulate dykes, although in this case the two sides of the plate will present the differences always seen in the upper and under surfaces of a bed of lava. The orientation-lines of five or six of the observed dykes were said to pass approximately through the axis of the cone of Vesuvius ; but all the rest presented great diversities, and some, when prolonged, would not touch the cone at all. In making a lithological examination of the dykes of Somma, the author directed particular attention to the position of the elongated air-bubbles found in the material of each dyke, considering that the direction of the longest axis of these bubbles would indicate the flow of the material when in fusion. He stated that on the whole the long axes of the bubbles are nearly horizontal or pointing at moderate angles upwards, in directions very nearly parallel to the plane of the dykes at the place where they occur. Hence he inferred that the dykes were filled by injection not from below, but nearly horizontally. The author further referred to the mineralogical characters of the materials of the dykes, and stated that they are not all composed of leucitic lava ; he also mentioned the occurrence of cross columnar structure in some of the larger ones. After referring to the differences observable in the physical condition of the two surfaces of some dykes, the author proceeded to consider the mode of origin of the fissures which, when filled, constitute volcanic dykes. He maintained that the production of a fissure and its filling with molten matter must have been simultaneous and due to the same cause, namely the hydrostatic pressure of the liquid lava more or less filling the crater, the pressure originating the fissure, into which the pressing liquid at the same time entered. A fissure thus produced and filled will always be widest near the crater ;



so that if the material of the cone were perfectly uniform, the dykes produced would be wedge-shaped. But, from the absence of this uniformity and other causes, fissures commenced at the interior and propagated into the mass of volcanic cones can rarely be uniformly distributed round the crater or produced in regular vertical planes in a truly radial direction. Hence the author concluded that it is unsafe to attempt to fix the position of an ancient crater by means of the intersection or concurrence of the lines of apparent orientation of dykes alone. The author stated that the intrusion of volcanic dykes cannot so greatly influence the slope of volcanic mountains as has been supposed.

### XLIX. *Intelligence and Miscellaneous Articles.*

ON THE REACTIONS OF METALLIC THALLIUM BEFORE THE BLOW-PIPE. BY E. J. CHAPMAN, PH.D., PROFESSOR IN UNIVERSITY COLLEGE, TORONTO\*.

THE following reactions are given from direct experiments by the writer†.

In the closed tube thallium melts easily, and a brownish-red vitreous slag, which becomes pale yellow on cooling, forms around the fused globule.

In the open tube fusion also takes place on the first application of the flame, whilst the glass becomes strongly attacked by the formation of a vitreous slag, as in the closed tube. Only a small amount of sublimate is produced. This is of a greyish white colour; but under the magnifying-glass it shows in places a faint iridescence.

On charcoal, *per se*, thallium melts very easily, and volatilizes in dense fumes of a white colour streaked with brown, whilst it imparts at the same time a vivid emerald-green coloration to the point and edge of the flame. If the heat be discontinued, the fused globule continues to give off copious fumes; but this action ceases at once if the globule be removed from the charcoal. A deposit, partly white and partly dark brown, of oxide and teroxide is formed on the support; but, compared with the copious fumes evolved from the metal, this deposit is by no means abundant, as it volatilizes at once where it comes into contact with the glowing

\* Communicated by the Author.

† The reactions given by Crookes are as follows:—"The metal melts instantly on charcoal and evolves copious brown fumes. If the bead is heated to redness it glows for some time after the source of heat is removed, continually evolving vapours which appear to be a mixture of metal and oxide. A reddish amorphous sublimate of protoperoxide surrounds the fused globule. When thallium is heated in an open glass tube it melts and becomes rapidly converted into the more fusible protoxide, which strongly attacks the glass. This oxide is of a dark red colour when hot, solidifying to a brown crystalline mass. The fused oxide attacks glass and porcelain, removing the silica. Anhydrous peroxide of thallium is a brown powder, fusing with difficulty and evolving oxygen at a red heat, becoming reduced to the protoxide. The phosphate and sulphate will stand a red heat without change."

charcoal. If touched by either flame, it is dissipated immediately, imparting a brilliant green colour to the flame-border. The brown deposit is not readily seen on charcoal; but if the metal be fused on a cupel, or on a piece of thin porcelain or other non-reducing body, the evolved fumes are almost wholly of a brownish colour, and the deposit is in great part brownish black. It would appear, therefore, to consist of  $\text{TlO}_3$  rather than of a mixture of metal and oxide. On the cupel thallium is readily oxidized and absorbed. It might be employed, consequently, as suggested by Crookes, in place of lead, in cupellation; but to effect the absorption of copper or nickel, a comparatively large quantity is required. When fused on porcelain, the surface of the support is strongly attacked by the formation of a silicate, which is deep red whilst hot and pale yellow on cooling.

The teroxide, as stated by Crookes, evolves oxygen when heated, and becomes converted into  $\text{TlO}$ . The latter compound is at once reduced on charcoal, and the reduced metal is rapidly volatilized with brilliant green coloration of the flame. The chloride produces the same reaction, by which the green flame of thallium may easily be distinguished from the green copper-flame, the latter, in the case of cupreous chlorides, becoming changed to azure blue. With borax and phosphor-salt, thallium oxides form colourless glasses, which become grey and opaque when exposed for a short time to a reducing-flame. With carbonate of soda they dissolve to some extent; but on charcoal a malleable metallic globule is obtained. The presence of soda, unless in great excess, does not destroy the green coloration of the flame.

Thallium alloys more or less readily with most other metals before the blowpipe. With platinum, gold, bismuth, and antimony respectively, it forms a dark grey brittle globule. With silver, copper, or lead, the button is malleable. With tin, thallium unites readily; but the fused mass immediately begins to oxidize, throwing out excrescences of a dark colour, and continuing in a state of ignition until the oxidation is complete. In this as in other reactions, therefore, the metal much resembles lead.

#### ON THE PHYSICAL PROPERTIES OF GALLIUM.

BY LECOQ DE BOISBAUDRAN.

I have recently prepared rather more than half a gramme of pure gallium. In the liquid state this metal is of a beautiful silvery whiteness; but in crystallizing it assumes a very pronounced bluish tinge, and its lustre notably diminishes.

By suitably working the solidification of overmelted gallium, isolated crystals are obtained: these are based octahedra, which I am occupied in measuring.

In a first trial (April 1876) the melting-point had been found to lie between  $29^\circ$  and  $30^\circ$ , or about  $29^\circ.5$ . I have just examined six specimens of gallium electrolyzed *successively from one and the same solution*. The foreign metals supposed to be present would be unequally distributed among the different fractions collected.

	Melting-point.
Gallium no. 1. ....	+30·14
„ no. 2. ....	+30·16
„ no. 3. ....	+30·14
„ no. 4. ....	+30·15
„ no. 5. ....	+30·16
„ no. 6. ....	+30·16
Mean .....	+30·15

The specimen no. 4 was next placed for two hours in boiling water; and the innumerable globules formed were united by compression. The melting-point had not varied at all. The metal was therefore certainly free from potassium.

The six specimens having been mixed, a fragment weighing  $2\frac{1}{2}$  centigrammes was taken and kept during half an hour in nitric acid diluted with its volume of water, at a temperature of  $60-70^{\circ}$ . The loss amounted to only a fraction of a milligramme. The melting-point had remained fixed; for the metal fused *very slowly* at  $+30^{\circ}16$ , and crystallized *very slowly* at  $+30^{\circ}06$ .

I shall shortly have the honour of submitting to the Academy the new process which has served for preparing this gallium, of which the constant melting-point denotes the very satisfactory purity.

In May 1876 I essayed to measure the density of the gallium, upon a specimen weighing 6 centigrammes; I obtained 4·7 at  $15^{\circ}$  (and relative to water at  $15^{\circ}$ ). The mean of the densities of aluminium and indium being 4·8 (to 5·1), the specific gravity provisionally found for gallium appeared to accord very well with a theory placing this metal between indium and aluminium. Nevertheless the calculations instituted by M. Mendeleef for a hypothetical substance which seems to correspond to gallium, at least as to several of its properties, conducted to the number 5·9.

Gallium crystallized under water decrepitates sometimes when it is heated. Perhaps my first metal contained vacuoles filled with air or water. I do not know if this cause of error was or was not associated with others to falsify my first determination; if so, however, I avoided it afterwards by strongly heating the metal, and solidifying it in a dry atmosphere. I then obtained higher densities—varying, however, from 5·5 to 6·2, while the weight of the portions taken did not exceed a few centigrammes.

Lastly, I have just operated with 58 centigrammes of gallium obtained by combining the six specimens above mentioned.

Density at  $+23^{\circ}$  (and relative to water at  $+23^{\circ}$ ).

1st experiment .....	5·90
2nd „ .....	5·97
Mean .....	5·935

The same gallium was afterwards kept during half an hour between  $60^{\circ}$  and  $70^{\circ}$  in nitric acid diluted with its volume of water, washed, pretty strongly heated, and, finally, solidified in dry air.

Density at  $+24^{\circ}45$  (and relative to water at  $+24^{\circ}45$ ) = 5·956.

It is, I think, unnecessary to insist on the extreme importance which attaches to the confirmation of the theoretic views of M. Mendeleef respecting the density of the new element.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxiii. pp. 611–613.

POSTSCRIPT TO PROFESSOR SYLVESTER'S "NOTE ON SPHERICAL HARMONICS."

The value of  $\phi(m, n)$  is stated inaccurately in the long footnote at pp. 302, 303. If

$$\Omega i = \frac{(2\pi)^i}{1.3.5 \dots (2i-1)}$$

and

$$R = \sqrt{1 - 2\Sigma h h' . t^2 + \Sigma h^2 . \Sigma h'^2 . t^4},$$

then I find

$$\phi(m, 2i+1) = \frac{(2i-1)\Omega i}{2m+2i-1};$$

and accordingly the Bipotential in space of  $2i+1$  dimensions is

$$\int_1^0 \frac{\Omega i dt^{2i-1}}{R^{2i-1}}.$$

Also I find that in space of  $2i+2$  dimensions the prospherical Bipotential is

$$\frac{2\pi^i}{1.2.3 \dots i} \int_1^0 \frac{dt^i}{(1 - 2\Sigma h h' . t + \Sigma h^2 . \Sigma h'^2 . t^2)^i}.$$

The above results may be extended to general quadric surfaces and prosurfaces. Thus, *ex. gr.*, if an indefinitely thin ellipsoidal shell be contained between two concentric surfaces, the equation to one of which is  $G(x, y, z) = 1$ , where  $G$  is a general quadric, and if the squared density at  $x, y, z$  is the reciprocal of

$$G(x-h, y-k, z-l) . G(x-h', y-k', z-l'),$$

then the mass of the shell divided by its volume is

$$\int_1^0 \frac{dt}{\sqrt{1 - At^2 + Bt^4}},$$

where

$$A = \Sigma \left( h \frac{d}{dx} \right) . \Sigma \left( h' \frac{d}{dx} \right) G(x, y, z),$$

and

$$B = G(h, k, l) . G(h', k', l').$$

It is further noticeable that if  $F$  and  $G$  are contravariantive forms, each numerator of the fractions expressing the differential derivatives of  $\frac{1}{\sqrt{G(x, y, z)}}$  is nullified by the operator

$$F \left( \frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz} \right);$$

and conversely, every rational integer function of  $x, y, z$  so nullifiable is a linear function of such numerators. And so in general the Theory of Spherical and Prospherical merges in a theory of Conicoidal and Proconicoidal Harmonics.—J. J. S.

Steamship 'Parthia,' Sept. 8, 1876.

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AND  
JOURNAL OF SCIENCE.

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[FIFTH SERIES.]

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DECEMBER 1876.

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L. *On Bi- and Unilateral Galvanometer Deflection.* By G. CHRYSTAL, B.A., Fellow of Corpus Christi College, Cambridge\*.

I WAS led to study the subject of this paper during a series of experiments undertaken for the purpose of directly testing Ohm's law.

The results arrived at are, I think, interesting, not only in connexion with galvanometry, but also in relation to the theory of induced magnetism. In the first instance I shall describe the phenomena as simply as possible from the first point of view, and then consider a little more closely some points which arise when the matter is looked at from the second point of view. This order is to a great extent that in which the facts came under my notice; and it has the additional advantage that it leads us incidentally to see that the phenomena in question really have their seat solely and entirely within the galvanometer, and have nothing to do with any phenomenon of the nature of unilateral conductivity or with any other exception to Ohm's law.

Dr. Schuster has described† an experiment in which a small current of constant direction is superposed on the alternating currents of a sine inductor, and the whole sent through a galvanometer. Such an experiment affords (under certain suppositions) a test of Ohm's law; for the average intensity of the current in the direction of the small constant current is greater than that in the opposite direction; hence, if the re-

\* Communicated by the Author.

† Phil. Mag. [IV.] vol. xlviii. p. 340.

sistance of the circuit depends on the intensity of the current, one of the currents will prevail, and the presence of the alternating currents will affect the permanent deflection due to the small constant current, whereas, if Ohm's law were true, there would be no such effect. Without discussing Dr. Schuster's results here, we may remark that a similar test of Ohm's law could be obtained by merely passing through a galvanometer the currents from the secondary coil of an inductorium. It is well known that if the primary be made and broken periodically, there will be an alternating current in the secondary, which will have the same period. The whole quantity of electricity which passes during a complete oscillation is zero; but the maximum intensities of the positive and negative parts of the current are very different. The positive part, starting immediately after the break, has a considerable initial intensity, which is independent of the resistance of the secondary; the negative part, starting at the make, begins, on the contrary, with zero intensity, and never reaches so large a maximum as the positive\*.

It follows that if the resistance of the circuit depends on the intensity of the current, then the two parts of the current will not experience equal resistances, and we shall get a galvanometer indication in the direction of that which has the advantage. Any such effect would be much increased by the introduction into the secondary circuit of a resistance composed of very fine wire. It is easy enough to calculate what this resistance should be, in order to produce the greatest effect on the galvanometer.

Such a resistance I used in the shape of a fine German-silver wire ( $\cdot 002$  inch diameter) wound on a cylindrical piece of vulcanite about 9 inches long, the turns being insulated from each other by the thread of a screw of one hundred turns to the inch cut in the vulcanite. The whole is enclosed in a glass tube with brass caps and copper terminals. For this instrument I am indebted to Mr. Garnett, of St. John's, Demonstrator at the Cavendish Laboratory. The induction-coil used was of the ordinary lecture-room form by Apps; the primary was made and broken by electric tuning-forks of various pitch.

The results I obtained indicated an *apparent* departure from Ohm's law, sometimes in one direction, sometimes in the other. The presence or absence of the fine wire in the circuit did not seem to be an essential condition of the phenomenon.

\* This supposes the period of alternation long compared with the time-constants of the coil; the same description applies, in a modified degree, to other cases.

I was therefore led to suspect that the cause lay in the galvanometer itself—a suspicion which became certainty when I found that reversing the galvanometer-connexions with the secondary, or reversing the primary, had no effect whatever on the character of the phenomena. It appeared that the effects observed could be analyzed as follows:—Suppose we are using a Thomson's galvanometer with mirror, lamp, and scale as usual, and let the scale be placed parallel to the coil-windings, a common perpendicular passing through the centre of the mirror, and the slit through which the light comes from the lamp; then two distinct states appear, according to the relation between the strength of the alternating currents in the secondary and the strength of the magnetic field in the axis of the coil due to the earth and deflecting magnet. 1st. If the currents are powerful enough and the magnetic field weak enough, the spot of light goes off the scale completely, either to one side or the other, and remains there. It can be made to go to either side and remain there by starting it off properly, which is easily enough managed by throwing on the alternating currents after it has passed the zero towards the side to which it is desired to send it. The spot will not remain at zero, even when placed there very carefully. This phenomenon I call *bilateral deflection*\*. 2nd. If the strength of the currents be *decreased* sufficiently, whether by interpolating resistance in the secondary or primary, or by reducing the electromotive force in the primary, or by shunting the galvanometer—or if, on the other hand, the strength of the magnetic field be sufficiently increased, say, by lowering the deflecting magnet,—

I. If the spot of light be brought, when there are no electrical oscillations, to zero on the scale, then on setting the coil in action it comes to rest at zero and remains steady there.

II. If the spot be brought to any position right or left of zero, then when the coil is in action it comes to rest in some position a little further to the right or left respectively, and remains steady there. The difference between these positions is greater the greater the original deflection of the spot from zero. This phenomenon I call *unilateral deflection*.

III. No difference of any kind was produced in any of these phenomena by reversing the connexions of the secondary with the galvanometer. Nor did the character of the phenomenon depend on the number of alternations per second, which in my experiments varied from 10 to 200. It was the second of the last-mentioned set of phenomena

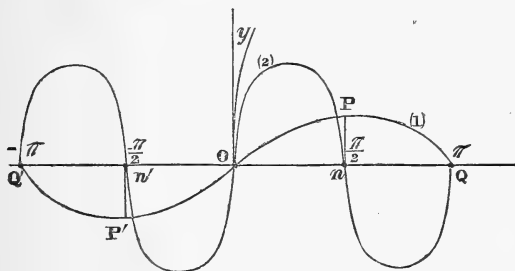
\* After Poggendorff, who originally observed the phenomenon and called it "Doppelsinnige Ablenkung."—Pogg. *Ann.* vol. xlv. p. 353 (1838).





for different relative positions. These points of intersection correspond to the positions of equilibrium of the needle ; and the stability or instability of the equilibrium is seen at a glance. For example, take the case  $\alpha=0$ . The points corresponding to  $\theta=0$  in (1) and (2) must be superposed ; thus, for a *large* value of B we have fig. 1. Here O, P, P', Q, and Q' give

Fig. 1.

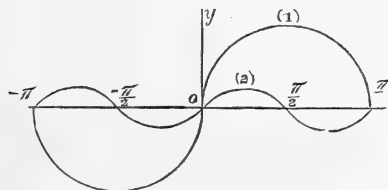


positions of equilibrium ; but O is obviously unstable, because on going to the right (increasing  $\theta$ ) the curve (2) lies above the curve (1), *i. e.* the force tending to increase  $\theta$  preponderates ; and similarly, if we go to left (decrease  $\theta$ ), the force tending to decrease  $\theta$  preponderates. Similarly, Q and Q' are unstable positions ; but P and P' are stable positions symmetrically situated with respect to O. The positions of P and P'

lie nearer a point distant  $\frac{\pi}{2}$  from O, the greater B. This case is best illustrated experimentally with a tangent-galvanometer of the usual construction, where the motions of the needle can be traced all the way round. We have thus explained Poggendorff's case, which is the limit to the state of the phenomenon\*.

Next, suppose B to be small, we have fig. 2. The positions

Fig. 2.

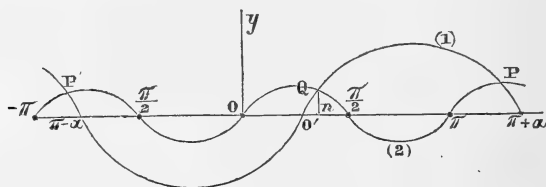


\* There are some points in Poggendorff's observations which did not appear in mine ; but the difference might arise from the inductive influence of the earth being sensible in his experiments, which was not the case in mine.

P and P' have now disappeared, and the equilibrium at O has become stable. This corresponds to case (1) of the second state of the phenomenon.

Lastly, suppose B small and  $\alpha$  not zero, then we must *displace* the origin in curve (1) to the *right*, say. We thus get fig. 3. Here  $O O' = \alpha$ , so that O' represents the position of

Fig. 3.



equilibrium when there are no currents. P and P' are *unstable*; and Q represents a single stable position when the currents are going. The deflection corresponding to this position is  $O n$ , where  $n$  is the foot of the ordinate of Q, and  $O n$  is  $> O O'$ , in accordance with the experimental facts above stated.

Similar reasoning would show that if the magnet had been deflected in the opposite direction, the alternating currents would have increased the deflection in the same direction. In fact the above simple graphical representation embraces the experimental facts, as I have observed them, completely.

It may render the above clearer to give the results of one of the earlier experiments with a Thomson's galvanometer. No care was taken to adjust the scale parallel to the windings of the coil; so that the results are not symmetrical; but they illustrate perfectly the nature of the phenomenon in its second state.

Position of spot with no current.	Position of spot with currents going.	Difference.
+173	+350	177
+ 50	+108	58
- 2	+ 10	12
- 14	- 15	1
- 20	- 30	10
- 64	-134	70
-193	-355	162

In this case the mirror was very nearly parallel to the coils when the spot of light was at -14 on the scale. It will be seen from the above that the effects are of a very decided and unmistakable character. I may also add that I have got similar results with three different galvanometers.

This may be sufficient so far as the subject is connected with galvanometry merely. I need hardly call attention to the importance of these phenomena in relation to experiments such as those of Dr. Schuster, where alternating currents are used.

In attempting to get a clearer insight into the nature of the effect on the magnetism of the needle, I was led to make some numerical verifications, and to give some variations to the experiment which may perhaps be of interest.

The phenomenon of magnetic induction is more or less complicated according to circumstances. We have to deal, in fact, with several distinct phenomena, which may for the present purpose be classified under two heads:—I. Temporary Magnetism; II. Residual Magnetism and its gradual decay. These phenomena are analogous to (1) the temporary strain, (2) the permanent set and “elastic recovery”<sup>\*</sup> in a solid subjected to stress in any way.

In what follows I shall assume that we have to do with temporary magnetism merely. Even when thus simplified the present case is to some extent peculiar. It is probable that the maximum magnetization producible by a given force is attained only after the force has been in action for some time †. Now, if the effects we are considering be due to induced magnetism at all, it is obvious that a very considerable fraction of the induced magnetism due to a given magnetic force must be developed in an interval of time incomparably smaller than the  $\frac{1}{400}$  of a second, whereas in the ordinary experiments on induced magnetism the time allowed for the development of the magnetization is practically unlimited. This peculiarity gives the present case additional interest.

I shall, in what follows, assume that when the magnetic force to which an element of iron or steel is subject varies, the corresponding variation of the magnetic moment of the element follows at an interval of time which is incomparably shorter than any other we have at present to deal with (*e. g.* the time during which either of the induced currents remains in the neighbourhood of its maximum).

With this assumption, we may apply the ordinary theory of magnetic induction. Three general conclusions may at once be drawn from the simple consideration that an elongated body tends to place its axis parallel to the lines of magnetic force:—

<sup>\*</sup> By elastic recovery is meant what in Germany is familiarly known under the name of “Elastische Nachwirkung.” I do not know of any English name for it which has the sanction of good authority.

† Wiedemann, *Galvanismus*, Bd. II. 2, p. 160. Also Faraday, *Phil. Mag.* [IV.] vol. ix. p. 92.

I. An elongated magnet magnetized axially *would* give phenomena analogous to those observed.

II. A spherical magnet would give *no such* phenomena.

III. An elongated magnet magnetized transversely would give similar phenomena, except that in unilateral deflection the sign of the effect would be reversed; *i. e.*, the spot of light being brought, by means of the deflecting magnet, to the right or left of zero, the effect of the alternating currents would be to *diminish* this deflection.

Conclusion III. was directly verified; and the result was in complete agreement with theory. Conclusion II. was also verified experimentally. A small spherical steel magnet was fitted with a mirror hung up in the galvanometer and observed, as will be afterwards described in the case of an elongated magnet magnetized axially. The magnetic moment of the sphere was roughly determined for me by Mr. Shaw, B.A., of Emanuel College; the maximum horizontal earth-couple on the sphere was about  $\cdot 22 \text{ grm.} \left( \frac{\text{cm.}}{\text{sec.}} \right)^2$ .

Two cells of Grove were used in the primary of the induction-coil. The result was a feeble *unilateral* deflection. An observation was made with an elongated magnet consisting of a piece of thin watch-spring magnetized longitudinally. The maximum earth's couple in this case was about  $\cdot 27 \text{ grm.} \left( \frac{\text{cm.}}{\text{sec.}} \right)^2$ . All the other arrangements were exactly as before. The result was strong *bilateral* deflection. Here the two magnets were very nearly in the same circumstances, the advantage being somewhat in favour of the sphere, owing to its smaller moment. It appears, then, that the form of the magnet has a very powerful effect on the phenomenon.

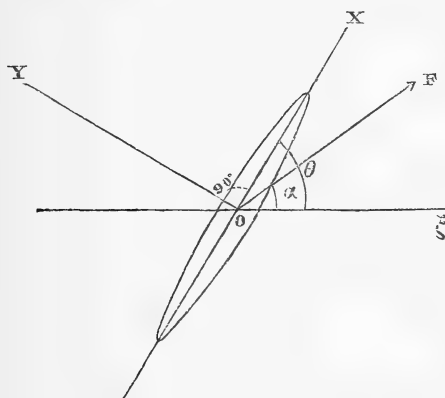
That the effect should be absolutely *nil* with the sphere was not to be expected; for we know\* that a piece of steel once permanently magnetized by a force  $F$  is never in the same state as it was originally. We can demagnetize the steel apparently completely by a force less than  $F$ ; but it requires a force greater than  $F$  to magnetize to an equal degree in the opposite direction. The particles of a magnetized steel sphere have therefore a *quasi*-crystalline structure related to the magnetic axis; so that the perfect symmetry which causes a sphere to behave neutrally in a field of uniform force, as far as induced magnetism is concerned, is probably in some degree lost, and can only be restored by heating the steel over red heat and

\* *E. g.* Rowland, *Phil. Mag.* [IV.] vol. 1. p. 358. Also Fromme, *Pogg. Ann. Ergbd.* vii. p. 421; *Phil. Mag.* [IV.] vol. 1. p. 299.

allowing it to cool apart from magnetic influence, or by some other equivalent process of molecular revolution.

A closer examination into the behaviour of an elongated magnet further confirms the above theory. Let us take as type of such a magnet a very elongated ellipsoid of revolution magnetized parallel to its axis, and suspended from a point in its equator. Let  $o\xi$  be the plane of the galvanometer-wind-

Fig. 4.



ings,  $oF$  the direction of the resultant magnetic force ( $F$ ) when there is no current,  $oX$  the direction of the axis of the magnet at any time,

$$\alpha = Fo\xi,$$

$$\theta = Xo\xi,$$

$m$  = permanent magnetic moment,

$\kappa$  = coefficient of induced magnetization,

$V$  = volume of magnet,

$i$  = current at any time  $t$ ,

$g$  = constant of galvanometer,

$n$  = number of breaks per second in primary.

The component forces parallel to  $oX$  and  $oY$  at time  $t$  tending to magnetize the needle inductively are

$$X = gi \sin \theta + F \cos (\theta - \alpha),$$

$$Y = gi \cos \theta - F \sin (\theta - \alpha).$$

Hence (see Maxwell's 'Electricity,' vol. ii. pp. 65 & 67) the couple tending to increase  $\theta$  is

$$V \left( \kappa - \frac{\kappa}{1 + 2\pi\kappa} \right) XY = \frac{\pi\kappa^2 V}{1 + 2\pi\kappa} \{ g^2 i^2 \sin 2\theta + 2Fgi \cos(2\theta - \alpha) - F^2 \sin 2(\theta - \alpha) \}.$$

Considering now what happens during a complete oscillation, let  $P$  be the uniform force whose action during that time is equivalent to the action of the varying force due to induced magnetism, then

$$P \frac{1}{n} = \frac{\pi \kappa^2 V}{1 + 2\pi \kappa} \left\{ g^2 \sin 2\theta \int_0^{\frac{1}{n}} i^2 dt - F^2 \cdot \frac{1}{n} \cdot \sin 2(\theta - \alpha) \right\}.$$

The middle term disappears because  $\int_0^{\frac{1}{n}} i dt = 0$ . Hence, if  $I^2$  denote  $\int_0^{\frac{1}{n}} i^2 dt \div \frac{1}{n}$ , i. e. the mean square of the induced currents, then

$$P = A'I^2 \sin 2\theta - B' \sin 2(\theta - \alpha), \quad (1)$$

where

$$A' = \frac{\pi \kappa^2 V g^2}{1 + 2\pi \kappa}, \quad B' = \frac{\pi \kappa^2 V F^2}{1 + 2\pi \kappa}.$$

Adding now the couple due to the permanent moment of the needle, we get for the whole force tending to *decrease*  $\theta$ ,

$$mF \sin(\theta - \alpha) - A'I^2 \sin 2\theta + B' \sin 2(\theta - \alpha). \quad (2)$$

In all the experiments of verification to be afterwards described (and in all the experiments discussed in this paper) the permanent field on the galvanometer was very much weakened by properly adjusting permanent magnets. Under these circumstances  $B'$  is very small compared with  $mF$  and  $A'I^2$ . It is easy to verify this by comparing the observed values of the times of oscillation of the needle when the currents are going and when they are not, with the values calculated by means of (2). The verification, however, is suppressed, as it is long and uninteresting.

The expression for the couple therefore becomes

$$mF \sin(\theta - \alpha) - A'I^2 \sin 2\theta, \quad (3)$$

the same as that obtained above by looser reasoning. It appears, then, in the first place, that if we make a series of observations of unilateral deflection, and if  $\alpha$  define the position of the needle for no current, and  $\theta$  the position when the currents are going, then, other things being equal,

$$\frac{\sin(\theta - \alpha)}{\sin 2\theta} = \frac{A'I^2}{mF} = \text{const} = C \text{ say.}$$

To verify this law I used the following arrangement:—The galvanometer (designed by Professor Maxwell and originally made by Warden, Clark, and Muirhead, but rewound by myself last summer) has two coils wound in channels  $\frac{7}{8}$  inch broad,  $1\frac{1}{16}$  inch deep, and 4 inches in external diameter. The number

of windings, partly of thin and partly of thick wire, is about 2668, making up a resistance of about 68 ohms. The channels are cut in the same piece of boxwood, along the axis of which is drilled a cylindrical hole in which hangs the magnet. The ends of this cylindrical cavity are closed by two caps, one of which is fitted with a plano-convex lens, the other with a piece of plane-parallel glass. The torsion-head is fitted with a Weber's suspension-screw in the usual way. The whole of the upper part is supported on a foot, with placing screws in such a way that the coils can be turned about a vertical axis.

The magnet consisted of a piece of silvered glass fitted to a brass frame, with weights for increasing the moment of inertia. To the back of this was fitted a thin piece of magnetized watch-spring about 10 millims. long and 2 millims. broad. Under the earth's force the period of the needle's oscillation was 22.2 sec. This was raised to 49.2 sec. by properly weakening the field.

The deflection of the magnet was measured by means of a scale and telescope in the ordinary way, the position of rest being deduced from three elongations. The deflection of the coils was measured by means of the faint image of the scale from the plate-glass cap which closes the cylindrical core.

The battery used was six cells of Smee; and the primary circuit was made and broken by means of an electric tuning-fork lent me by Mr. Dew Smith, driven by the primary current itself. The induction-coil was of the ordinary construction, the resistance of the secondary being about 2714 ohms.

By making an observation with the planes of the plate-glass cap and mirror very nearly parallel, the value of the correction for the deviation of magnetic axis of needle in this position from the true plane of the coils was found. Using this correction, a series of observations with different deflections of the coils gave the following result:—

$\alpha$ .	$\theta$ .	C.
+1 28	1 55	·1175
+4 0	5 13	·1172
+5 0	6 29	·1156
+5 16	6 54	·1195
-4 34	6 3	·1225

Considering the means employed in obtaining them, these values of C, differing from the mean by less than 4 per cent., agree as well with each other as was to be expected. The two disturbing elements were the inconstancy of the battery and

the varying residual magnetism of the iron core in the induction-coil.

I next made some experiments to determine whether, other things being equal,  $C$  varies as  $I^2$ , which by the above theory it ought to do. With this object in view different resistances were interpolated in the secondary, every thing else being kept the same. The resistance of the secondary, including the galvanometer, was about 2768 ohms. Resistances of 1000 and upwards were put in,  $\alpha$  and  $\theta$  observed, and the resulting values of  $C$  calculated.

If we suppose the time which elapses between two successive interruptions of the primary to be so long that the current in the primary arrives at the steady state in the interval, and the induced currents in the secondary due to make and break do not interfere, then it is very easy to calculate the value of  $I^2$  for the induction-currents. The result is

$$I^2 = \frac{nM^2j^2}{2NQ} \left\{ 1 + \frac{NP}{LQ + NP} \right\}, \quad \dots \quad (A)$$

where  $L$ ,  $N$  are the coefficients of self-induction for the primary and secondary,  $M$  the coefficient of mutual induction, and  $P$  and  $Q$  the respective resistances,  $j$  the steady current in primary, and  $n$  the number of interruptions per second as before. Of the two terms within the bracket the first is contributed by the current due to the break, the second by that due to make.

Now with an induction-coil such as I used—where  $L = \cdot 013$ ,  $M = \cdot 79$ ,  $N = 52$ , and  $P = 2$  (say) and  $Q = 2768$  (these numbers are very rough estimates deduced from experiments performed for practice)—the time-constants of the coil are such that with tuning-forks such as I used for producing the break (which gave  $n = 50, 100$ , or  $200$ ) the above formula is very far from being applicable.

In fact the result is much nearer what we should get by assuming that the primary current followed the sine law, in which case we should get,  $A$  being the maximum electromotive force in primary, and  $\nu = 2\pi n$ ,

$$I^2 = \frac{\frac{1}{2}\nu^2 M^2 A^2}{(LN - M^2)^2 \nu^4 + (N^2 P^2 + 2M^2 P Q + L^2 Q^2) \nu^2 + P^2 Q^2}, \quad (B)$$

in which case it is easy to see that if  $\nu$  be big enough, the effect on  $I^2$  of doubling and trebling the resistance  $Q$  will be comparatively small. This is confirmed by experiment, as the following Table will show:—



Q.	Values of C for different values of $n$ .		
	50.	100.	200.
2780	·2448	·2211	·2128
3780	·2359	·2126	·2062
8780	·1798	·1829	·1822
12780	·1424	·1568	·1657

When the value of  $n$  is less, the value of  $C$  ought to fall quicker as  $Q$  increases. This is confirmed by the following result of a series of experiments in which  $n$  had the value 10 very nearly :—

Q.	C.	D.
2780	·0774	1237
5480	·0475	1637
10480	·0242	1774
12780	·0194	1796

The column D gives the first four figures of the reciprocal of

$$\frac{1}{CQ} \left\{ 1 + \frac{NP}{NP + LQ} \right\},$$

which ought to be constant if  $I^2$  followed formula (A). It will be seen that D is not very far from being constant, the differences getting less as  $Q$  increases. This is what we should expect.

If instead of using the induction-coil in the usual way we throw the battery into the coil usually used as the secondary, and put the galvanometer into the small-resistance coil commonly used as the primary, then the induced currents, even with the 200 fork, will not interfere. We must in this case put in formula (A), which is now perfectly applicable,

$L=52$ ,  $M=.79$ ,  $N=.013$ ,  $P=2720$  (say),  $Q=68$ , the last arising practically from the galvanometer resistance. Hence

$$\frac{NP}{LQ + NP} = .010,$$

and when  $Q$  is doubled its value is .005 ; so that the fraction contributed by the current at make to the value of  $I^2$  is now comparatively small. Hence  $C$  will approximately vary inversely as the resistance  $Q$ , and  $C$  ought to drop to nearly half its value when  $Q$  is doubled.

The experiment was tried with twenty-five small Leclanché cells in the secondary of the induction-coil ; the breaking-fork

was now driven by an auxiliary battery. The value of  $n$  was 50. The galvanometer (resistance about 68) was put in the primary as above described.

The above calculation represents the case thus realized pretty closely; for although the self-induction of the galvanometer has been neglected and the resistance of the battery only roughly estimated, yet neither of these affects the important term. The result, therefore, of experiment ought to be nearly as above predicted. It was so as nearly as could be seen, taking account of unavoidable experimental errors.

It appears, therefore, that the above theory stands so far the test of experiment. When I can get the use of a sine-inductor or a sufficiently delicate electro-dynamometer (both of which will probably soon be added to the collection of instruments at the Cavendish Laboratory), it will be easy to test the theory still further.

If it be accepted, it seems to me that an interesting conclusion follows, viz. that, of the total induced magnetism which a given field of force is capable of generating in any body placed in it, a very considerable fraction must be developed in a time very much less than  $\frac{1}{400}$  of a second.

Perhaps a method for measuring the inductive capacities for temporary magnetism of *strongly* magnetic substances might be built on the experiments I have described; but this can hardly be done until it is better known what degree of accuracy can be ascribed to the law

$$C \propto I^2.$$

Possibly by sufficiently increasing the speed of revolution we might with a sine-inductor be able to introduce the element of *time* into magnetic measurements, and thereby get new light on the difficult subject of magnetic induction.

Cavendish Laboratory, Cambridge,  
October 2, 1876.

LI. *Attempt at a Theory of the (Anomalous) Dispersion of Light in Singly and Doubly Refracting Media.* By Professor E. KETTELER.

[Continued from p. 345.]

7. **I**F we now make the attempt to extend our theory to anisotropic media also, only one procedure will lead to the end in view, and that totally different from the usual one. According to Fresnel's method, namely, the mathematical treatment has hitherto been restricted exclusively to the differential equations of the vibratory motion of the æther perpendicular to the normal of the waves; and by means of them the

velocity-surface of the normal (corresponding to the "first ellipsoid"), as the primary, has been derived. Making use of this latter, the velocity-surface of the rays, the "wave-surface" (corresponding to the second or Plücker's ellipsoid), is then obtained as the envelope of the same. The wave-surface thus appears only as secondary or derived from the former, while in nature it is quite the reverse: here the wave-surface only has a physical meaning, and the normal surface is associated with it solely as an (of course valuable) auxiliary surface.

The incorrectness referred to can only be avoided by admitting also the vibrations of the corporeal particles.

Let the medium whose doubly-refracting properties are in question have resulted from an isotropic one, with molecular distance  $r_0$  equal on all sides, through being exposed in three perpendicular directions to the pressure- or pull-forces  $p_x, p_y, p_z$ , and thereby attained linear extensions exactly proportional (as we will assume) to these pressures, which then have for their consequence the coordinate-distances  $x = x_0(1 + \alpha p_x) = x_0(1 + \alpha)$ ,  $y = y_0(1 + \alpha p_y) = y_0(1 + \beta)$ ,  $z = z_0(1 + \alpha p_z) = z_0(1 + \gamma)$ . The variable molecular distance  $r$  for any direction whatever, which forms with the axes of pressure the angles  $a, b, c$ , is calculated therefrom (as I will further on show) by means of the equation

$$\frac{1}{r^2} = \frac{\cos^2 a}{r_0^2(1 + \alpha)^2} + \frac{\cos^2 b}{r_0^2(1 + \beta)^2} + \frac{\cos^2 c}{r_0^2(1 + \gamma)^2}$$

If now an æther-point of the medium is permanently shaken by any external force, this motion is propagated to all the surrounding æthereal and corporeal particles; and after the lapse of, say, the unit of time, it has proceeded as far as a surface called  $\kappa\alpha\tau' \epsilon\xi\omicron\chi\eta\nu$  the wave-surface. Along each radius vector of this surface (a "ray") the æthereal and corporeal particles are therefore in associated motion. The conditions, however, of this association are, in my opinion, the following:—

a. The vibrations of the æthereal and corporeal particles necessarily take place in the plane given by the ray and the wave-normal, which therefore at the same time appears as a certain plane of symmetry of the medium.

b. The vibrations of the æther particles (which latter, on account of their minuteness, we conceive as at least approximately continuous) lie, by virtue of the incompressibility of the particles, within the tangential wave-plane.

c. On the contrary, the force which results from the resistance of the more discretely situated corporeal particles stands

perpendicular to the ray; and so far they do not behave otherwise in anisotropic than in isotropic media.

*d.* On the other hand, again, on account of the difference intimated, it does not appear indispensable that the corporeal and æthereal particles should vibrate in parallel directions. We preliminarily leave it undecided whether the vibrations of the former, so far as they have generally a regular direction, are perpendicular to the ray or to the normal.

This supposed, let  $e$  be the constant of deformation of pure æther; and let  $E, E', K$  be the constants of the additional forces, originating from the resistance of the corporeal particles and acting perpendicular to the ray.

Consequently, as regards the equation of motion of the æthereal particles vibrating perpendicular to the normal, we have, if  $e$  acts exactly in this direction and to  $e$  the increment  $E \cos \delta$  (understanding by  $\delta$  the angle between the ray and the wave-normal) is added,

$$m \frac{d^2 \rho}{dt^2} = (e + E \cos \delta) \frac{d^2 \rho}{dx^2}. \quad . \quad . \quad . \quad (15 \text{ A})$$

On the other hand, for the corporeal particles the previous differential equation

$$m' \frac{d^2 \rho'}{dt^2} = E' \frac{d^2 \rho'}{dx^2} + K \rho' \quad . \quad . \quad . \quad (15 \text{ B})$$

remains afterwards as before; and in it  $E'$  and  $K$  are to be referred to a direction perpendicular to the ray. We also again make the assumption that

$$E = \alpha e, \quad E' = \alpha e', \quad K = \alpha \kappa. \quad . \quad . \quad . \quad (16)$$

To integrate these equations, we imagine the actual excursion  $\rho$  (A) as a component of the virtual excursion  $\rho_0$  ( $A_0 = \mathcal{A}$ ), and put

$$\left. \begin{aligned} \rho_0 &= \mathcal{A} \cos 2\pi \left( \frac{t}{T} + \frac{x}{l'} - \Theta \right) = \frac{\rho}{\cos \delta}, \\ \rho' &= \mathcal{A}' \cos 2\pi \left( \frac{t}{T} + \frac{x}{l'} - \Theta \right). \end{aligned} \right\} \quad . \quad . \quad (17)$$

Here, consequently, as may be particularly remarked, the abscissæ  $x$  also are referred to the direction of the ray; and just so the  $l'$  are the internal wave-lengths measured in the same direction, not in the direction of the normal.

If, finally, we denote the ray-velocity by  $\sigma$  (in contradistinction to the wave-velocity  $\omega$ ), and put, besides the sine-ratio  $n = \frac{\sin e}{\sin r} = \frac{v}{\omega}$ , the ratio of velocities  $n' = \frac{\sigma}{\omega}$ , we obtain for

the latter, analogous to what was obtained before,

$$\frac{A'^2}{A_0^2} = \frac{\frac{\epsilon}{\epsilon'} \cos \delta}{L^2 - 1}, \quad n'^2 - 1 = \frac{\Sigma m' A'^2}{m A_0^2}, \quad \dots \quad (18)$$

if we generalize as much as possible by introducing the symbol of summation.

8. The problem that now remains is, to express the values  $\frac{\epsilon}{\epsilon'}$ ,  $\frac{\epsilon}{\kappa'} = L^2$  as functions of the variable molecular distance  $r$ ; while  $m'$  and  $m$  remain, as masses of cubic space, independent of any orientation.

It may first be asked, Which linear density of the corporeal structure comes into consideration in relation to the resistance to the vibrations of the æther? that in the direction of the giving-way of the corporeal particles, or that in the direction of the ray, or that in a third direction perpendicular to the two mentioned? We shall unhesitatingly select the *first*.

Further,  $\epsilon'$  and  $\kappa'$  both depend generally, complementary to each other, on the form, the chemical quality, and the forces of the molecular combination. It hence appears probable that every alteration of density will affect the one quantity as well as the other. In fact the result of my previous memoirs is, that the quotient  $\frac{\epsilon'}{\kappa'} = L^2$  is not merely independent of the cubic density for gases and liquids, but also has an identical value for the two or three principal indices of refraction of anisotropic media. On the other hand, it is different for calc-spar and aragonite notwithstanding their similar chemical composition. *We shall therefore regard our constant  $L^2$  as connected solely with the optico-chemical quality.*

The quotient  $\frac{\epsilon}{\epsilon'}$ , on the contrary, as the ratio of two quantities belonging to one another, of deformation of the æther and the corporeal particles, will necessarily change with the molecular distance  $r$  of the latter. Now, since for  $r$  infinitely great (which of course implies that  $m' = 0$ )  $n$  is equal to 1 and an increase of  $n'$  is united with the diminution of  $r$ , therefore  $\frac{\epsilon}{\epsilon'}$  will be inversely proportional, at least approximately, to some power of this distance. We select, for obvious reasons, the *first*; and if we thus put

$$\frac{\epsilon}{\epsilon'} = \frac{b}{r}, \quad \dots \quad (19)$$

understanding by  $\mathfrak{b}$  an absolute constant, we have experiment on our side, inasmuch as it is known that  $\frac{n'^2-1}{m}$  in the grand total changes but little with the density.

Therefore only the value of  $\delta$  still remains to be cleared up. For this purpose we will imagine the following experiment realized:—

Upon the plane dividing-surface of an isotropic substance falls, at the angle of incidence 0, the plane of a linearly polarized wave—that is, a pencil of an infinite number of parallel rays. It will enter the interior without refraction; and its polarization will remain, within as without, the same. Of the penetrating rays we take one, and imagine the corporeal particles with which it comes into contact in its path characterized by some external token.

We then compress or dilate the medium in two directions perpendicular to one another (but which, for simplicity's sake, shall both be parallel to the vibration-plane) unequally. The result is twofold. On account of the unequal axial extension, all the rows of molecules which do not fall into the direction of this axis are rotated a certain measurable angle, and among them the line before indicated, whose previous angle  $\chi_0$  with one of the force-directions changes into  $\chi_0 + \delta_0 = \chi$ . Secondly, the previously singly refracting medium becomes optically uniaxial for the plane considered; the refracted (extraordinary) wave-plane now corresponding to the incidence-angle 0 certainly remains parallel to the incident wave-plane; but the ray belonging to it appears, with respect to the incident ( $\chi_0$ ), likewise rotated through some angle  $\delta$ . Thus, in consequence of the modification taken, with one and the same line of space  $\chi_0$  the two new directions  $\chi_0 + \delta_0 = \chi$  and  $\chi_0 + \delta = \chi'$  would be associated. If at first one of the axes of pressure is made to coincide with the incident ray so as to make  $\chi_0 = 0$ , then  $\delta = \delta'$  also becomes  $= 0$ . If  $\chi_0$  be then increased,  $\delta_0$  and  $\delta$  will simultaneously increase: for the vicinity of  $\chi_0 = 45^\circ$  they reach their maximum, and sink again to 0 for  $\chi_0 = 90^\circ$ .

For this peculiar behaviour of the two directions  $\chi$  and  $\chi'$  (one of which is, besides, conditioned by the coexistence of the other) there is, in my opinion, no other satisfactory solution but just the postulate  $\chi = \chi'$ ,  $\delta_0 = \delta$ . According to this the angle  $\delta$  between wave-normal and ray (or the virtual vibration-directions corresponding to them) would be the same as the angle between the former direction as that of some particles of the unmodified medium and the direction of the same particles after the modification. Or, in other words, *The internal ray corresponding to one determinate external*

wave appears to be connected with the cooperation of the identical corporeal particles, whether the medium be isotropic, or by external forces transferred into the anisotropic state\*. The inference from such external forces to molecular forces is sufficiently obvious.

If, moreover, the medium is a compound, so that the summation-symbol of equation (18) comes into use, we may provisionally content ourselves with the special case that  $\delta$  is the same for all its optico-chemical elementary constituents, and therefore  $\cos \delta$  can be placed before the symbol of summation.

9. Assuming this,  $r$  and  $\delta$  can be calculated in the following manner:—Given the distance  $R_0$  of any particle of the unmodified medium from the origin of coordinates; let the line which joins the two (perpendicular to the above-noticed row of molecules) make with the axes the angles  $a_0, b_0, c_0$ . We have then

$$\cos a_0 = \frac{x_0}{R_0}, \cos b_0 = \frac{y_0}{R_0}, \cos c_0 = \frac{z_0}{R_0}.$$

In consequence of the modification the same row of molecules arrives at the somewhat different position  $a, b, c$ , and the distance  $R_0$  becomes  $R$ . Correspondingly there are then  $\cos a = \frac{x}{R}, \cos b = \frac{y}{R}, \cos c = \frac{z}{R}$ . For  $\delta, R$ , and  $R_0$  the relations now hold good:—

$$\cos \delta = \cos a_0 \cos a + \cos b_0 \cos b + \cos c_0 \cos c;$$

$$R^2 = x^2 + y^2 + z^2; \quad R_0^2 = x_0^2 + y_0^2 + z_0^2.$$

Now according as, by means of the axial distances of § 7, we reduce either the angle of the new direction to that of the old, or the angle of the old to that of the new, noticing the proportionality of  $R, R_0$  to  $r, r_0$ , we get the following expressions:—

$$\begin{aligned} r \cos \delta &= r_0(1 + \alpha) \cos^2 a_0 + r_0(1 + \beta) \cos^2 b_0 + r_0(1 + \gamma) \cos^2 c_0, \\ r^2 &= r_0^2(1 + \alpha)^2 \cos^2 a_0 + r_0^2(1 + \beta)^2 \cos^2 b_0 + r_0^2(1 + \gamma)^2 \cos^2 c_0; \end{aligned} \quad (20)$$

\* To the above consideration the theory of aberration (Ketteler, *Astron. Aberrationslehre*, p. 177) presents the following analogy:—Let a ray AB, incident upon an isotropic or anisotropic medium at rest, produce in its interior the refracted ray BC; and let the three points A, B, C be imagined as fixed once for all by three dioptrics inseparably connected with the structure of the medium—or, better still, B, C by an infinitely thin ideal tube led through the ponderable particles. The refracted light will pass through the tube without striking against its sides, even when the medium is moved in space with any velocity of translation whatever. And yet, in consequence of the motion, the angle of incidence, angle of refraction, and ratio of refraction change.

or

$$\left. \begin{aligned} \frac{\cos \delta}{r} &= \frac{\cos^2 a}{r_0(1+\alpha)} + \frac{\cos^2 b}{r_0(1+\beta)} + \frac{\cos^2 c}{r_0(1+\gamma)}, \\ \frac{1}{r^2} &= \frac{\cos^2 a}{r_0^2(1+\alpha)^2} + \frac{\cos^2 b}{r_0^2(1+\beta)^2} + \frac{\cos^2 c}{r_0^2(1+\gamma)^2}. \end{aligned} \right\} \dots (21)$$

If the penultimate of these be introduced into equation (18), and at the same time we put for shortness

$$\frac{b}{r_0(1+\alpha)} = \left(\frac{\epsilon}{\epsilon'}\right)_1, \quad \frac{b}{r_0(1+\beta)} = \left(\frac{\epsilon}{\epsilon'}\right)_2, \dots,$$

the equation takes the following definitive form:—

$$n'^2 - 1 = \Sigma \frac{\frac{m'}{L^2}}{\frac{m}{L^2} - 1} \left\{ \left(\frac{\epsilon}{\epsilon'}\right)_1 \cos^2 a + \left(\frac{\epsilon}{\epsilon'}\right)_2 \cos^2 b + \left(\frac{\epsilon}{\epsilon'}\right)_3 \cos^2 c \right\}. \quad (22)$$

If, finally, we epitomize the three sums as A, B, C, it is written more briefly,

$$\left. \begin{aligned} n'^2 &= (1+A) \cos^2 a + (1+B) \cos^2 b + (1+C) \cos^2 c, \\ \frac{1}{\sigma^2} &= \frac{\cos^2 a}{\omega_1^2} + \frac{\cos^2 b}{\omega_2^2} + \frac{\cos^2 c}{\omega_3^2}; \end{aligned} \right\} \quad (23)$$

and in this form it may have henceforth to replace the “second” or Plücker’s ellipsoid ( $\mathfrak{E}$ ), the expression of which has hitherto been  $\frac{\cos \delta}{r} = \frac{1}{\sigma}$ .

With the ellipsoid represented by equation (23) a second is then associated, represented by

$$\left. \begin{aligned} \frac{1}{n^2} &= \frac{\cos^2 a_0}{1+A} + \frac{\cos^2 b_0}{1+B} + \frac{\cos^2 c_0}{1+C}, \\ \omega^2 &= \omega_1^2 \cos^2 a_0 + \omega_2^2 \cos^2 b_0 + \omega_3^2 \cos^2 c_0. \end{aligned} \right\} \dots (24)$$

This may take the place of what has hitherto been named the first or reciprocal ellipsoid ( $\mathfrak{E}$ ), for which it has up to the present been assumed that  $r \cos \delta = \omega$ .

With the two planes mentioned the further theory of double refraction is, as is known, completely traced.

10. Up to this point we have held fast to the special case for compound media, that the angle between the ray and the wave-normal has the same value for all the individual optico-chemical elements; we can now drop this supposition. Even with the most common composition of the medium, the oscillatory motion about a point of it will, after the lapse of the time-unit, have advanced as far as to a perfectly determined closed plane. To a determinate radius vector of the same cor-





the length  $A$ , parallel to  $NO$ , towards the position of equilibrium, and carry with them the corporeal particles in paths the equivalent length of which will be  $A'$ . If for the position of equilibrium all the elasticity is transformed into *vis viva*, this resolves itself as before into  $m \frac{A^2}{T^2} + \Sigma m' \frac{A'^2}{T^2}$ .

If, on the other hand, in pure æther the same initial displacement is produced in order to attain the same elasticity, we have, in order that we may replace the interfering external force by a wave-motion of the same amplitude  $A$  and the same deviation, to bring the above parallelogram, by twisting its sides, into the form of a rectangle, and also to shorten its length  $l'$  to  $l$ , while  $l$  must be made  $= l' \cos \Delta$ . The maximum *vis viva* corresponding to this displacement is

$$m \frac{A^2}{T_0^2} = m \frac{A^2}{T^2} n^2 = \frac{e}{l^2} A^2.$$

The same end, however, is attained by displacing the æther particles  $A_0$ , while retaining the same extent  $l'$ , and forming a rectangle out of  $A_0$  and  $l'$  instead of  $A$  and  $l$ . It is precisely  $\frac{e}{l'^2} A^2 = \frac{e}{l'^2} A_0^2$ . Accordingly we get the coordinated relations :—

$$\left. \begin{aligned} \frac{m}{T^2} A^2 + \Sigma \frac{m'}{T^2} A'^2 &= \frac{e}{l^2} A^2; & n^2 - 1 &= \frac{\Sigma m' A'^2}{m A^2}; \\ \frac{m}{T^2} A_0^2 \cos^2 \Delta + \Sigma \frac{m'}{T^2} A'^2 &= \frac{e}{l'^2} A_0^2; & n'^2 - \cos^2 \Delta &= \frac{\Sigma m' A'^2}{m A_0^2} \end{aligned} \right\} \quad (26)$$

The first two have the same form as those of isotropic media ; but they do not contain, like the dispersion-formula (22), the velocity-ratio  $n' = \frac{v}{\sigma}$  and the corresponding internal wave-

length  $l'$  (measured on the ray), but the sine-ratio  $n = \frac{\sin e}{\sin r} \left( = \frac{v}{\omega} \right)$  and the wave-length  $l$  associated with it (measured on the normal), and lastly, not the full amplitude of the æther particles  $A_0$  perpendicular to the ray, but its value reduced to the height of descent  $A$  perpendicular to the normal.

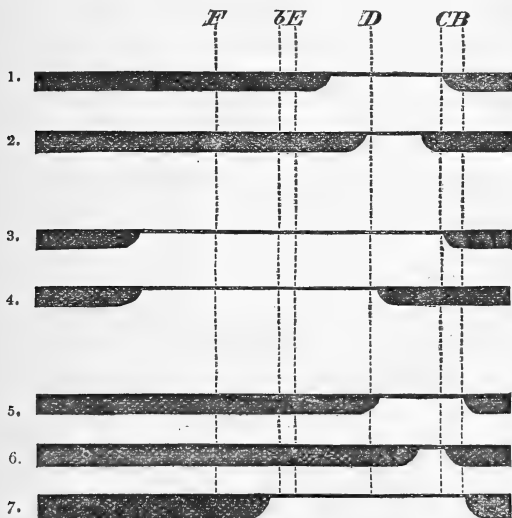
This law of the *actual vires vivæ* forms therefore the complement to the expression of the *virtual vires vivæ* in equation (18).

[To be continued.]

LII. *Selective Absorption.* By WILLIAM ACKROYD\*.

AT present much attention is being paid to absorption-spectra for technological as well as purely scientific reasons; but in the absence of classification and of a general theory accounting for the facts observed, the whole subject is surrounded by a haze which it were well for progress to dispel. Much perplexity and not a few mistakes have arisen from the confusing of two kinds of absorption, or at least two different manifestations of the same phenomenon, which hitherto have not been sufficiently contradistinguished. These we propose to classify on the basis of the following typical experiments:

1. Light is transmitted through a thin layer of potassic dichromate at the normal temperature, and again at a little below its fusing-point. We obtain spectra approximately represented by Nos. 1 & 2. It will presently be shown that this increase



of absorption at the higher temperature is the result of structural alterations in the dichromate. We shall therefore call this kind of absorption *structural absorption*.

2. A glass cell, 15 centims.  $\times$  3 centims., contains cupric sulphate in solution. Light sent through the 3-centims. direction gives spectrum No. 3, whilst light transmitted through the 15-centims. direction gives spectrum No 4. We have here

\* Communicated by the Physical Society.

increase of absorption without any structural alterations in the absorbing medium; for the cupric sulphate remains the same during the two observations, and the temperature is unaltered. The only difference is one of extent of medium. For this kind of absorption we propose, at the suggestion of Dr. Guthrie, the term *transverse* absorption.

Professor Gladstone\*, when speaking of the action of heat on coloured solutions, thus tacitly refers to the two kinds of absorption:—"The elevation of temperature seems merely to heighten the absorbent power of the dissolved salt, so that light absorbed by a certain quantity of the heated solution is the same as would have been absorbed by a larger quantity of the same solution if cold." Notwithstanding this, the two kinds of absorption seem not to have been formally separated; and the mixed inferences from one and the other have naturally produced a confusion in the minds of students which has made selective absorption a subject little understood. We shall confine our observations to structural absorption.

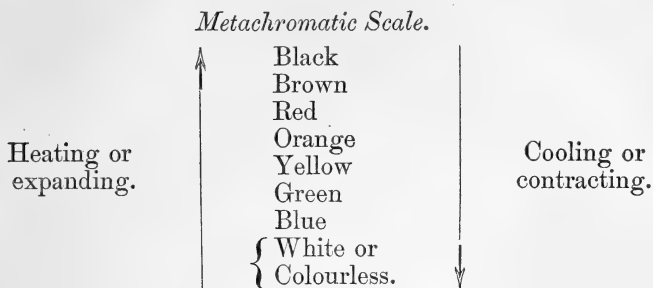
*Structural Absorption*.—Alteration of structural absorption upon elevation of temperature is, as a rule, obvious to the eye as a change of colour. This colour-change, for which I have elsewhere proposed the name of metachromatism, has been studied by Schönbein, Gladstone, Houston and Thompson, and, lastly, by myself. Many theories have been offered to account for the phenomenon; these were discussed in my paper on Metachromatism†, read before the Chemical Society on the 3rd of February last.

If a little oxide of zinc be heated strongly on a piece of white porcelain, it changes from white to orange and yellow. Chromate of lead treated similarly changes from orange to deep orange, brick-red, and black-red. These bodies quickly regain their original colour with a return of the normal temperature; hence one might suppose that the chromate of lead, if cooled far below the normal temperature, would acquire in succession the yellow and white of the zinc oxide, thus (in the order of cooling)—black-red, brick-red, deep orange, orange, yellow, and white.

Such a sequence of colour-change is exhibited by nitric peroxide; a ruddy vapour at the normal temperature, it becomes orange, yellow, and below zero a colourless liquid. From a study of such metachromatic facts and their sequence we have arrived at the following scale of change:—

\* Phil. Mag. [IV.] vol. xiv. p. 423.

† Chemical News, vol. xxxiv. pp. 76, 77.



The scale may be illustrated by heating the following anhydrous bodies on white porcelain. Zinc oxide changes from white to yellow and orange; metaborate of copper from blue to green and yellowish green; baric chromate from yellow to orange; mercuric oxide from orange-yellow to orange, red, and brown; potassic dichromate, red to dark red; trimanganic tetroxide, reddish brown to nearly black; and cuprous oxide glass from scarlet to dark scarlet and black.

Respecting this colour-change, we make out:—(1) that it arises from increased structural absorption with elevation of temperature, the more refrangible increment increasing at a greater rate than the less refrangible; and (2) that the only necessary concomitant is alteration of atomic potentiality—a change from the white towards the black end of the metachromatic scale signifying atomic recession, and a change from the black towards the white end, atomic approach.

(1) In support of this we must refer to the bichromate experiment. The change from red to dark red viewed spectroscopically gives spectra Nos. 1 and 2.

(2) To arrive at the approximate cause, I have heated colour-changing bodies in various media, as nitrogen, carbonic anhydride, and hydrogen, the result of which has been to show that the surrounding medium has nothing to do with metachromatism. The phenomenon is exhibited by all three states of matter—by solids (amorphous and crystalline), by liquids, and by gases near their liquefying-points. By a process of elimination it may readily be seen that the phenomenon is due to the reception of energy from the heating agent. Now of this transferred kinetic energy a portion is converted into energy of position. In the metachrome or colour-changing body we have therefore—

- i. Kinetic energy, and the complementary
- ii. Potential energy. This energy of position is acquired by
  - α. The overcoming of cohesion (molecular recession or

molar expansion), as, *e. g.*, in the conversion of ice into water, and water into steam, *work which is accompanied by a change of density*.

- β. The overcoming of chemical attraction (atomic recession or molecular expansion), which finally ends in decomposition, as, *e. g.*, in the resolution of  $\text{PtCl}_4$  into  $\text{PtCl}_2$  and  $\text{Cl}_2$ .

I. Now metachromatism is not confined to the coloured bodies one meets with in the laboratory; for many anhydrous silicates exhibit colour-change, and in some the change is permanent after the metachrome has been subjected to a high temperature. This is shown in the following Table\* in each example save that of olivine.

		Density before ignition.	After ignition.
1. Olivine . . . .	Pistachio-green . .	3.389	Pist.-green . . 3.378
2. Beryl . . . . .	Straw-yellow . . . .	2.697	Blue . . . . . 2.697
3. Topaz . . . .	Sherry . . . . .	3.539	Pink . . . . . 3.533
4. Zircon . . . .	Brown . . . . .	4.515	White . . . . . 4.540
5. „ . . . .	Aurora-red . . . . .	4.863	Colourless . . 4.863

If we might assume that *two bodies of identical chemical composition and density* have at a common temperature the same amount of kinetic energy, then it is evident that we may eliminate kinetic energy from the list of possible causes. For, to take the beryl (2), we have differences of colour (that is, of structural absorption) at a common temperature, and the same in the zircon 5, without alterations of density or chemical composition.

The behaviour of mercuric iodide is perhaps a less equivocal demonstration of this point. Examined spectroscopically at (say)  $16^\circ \text{C}$ ., a band of red light is transmitted extending from B to D. This narrows as the temperature rises (in other words, there is an increase of structural absorption), up to about  $140^\circ \text{C}$ . The band of transmitted light now suddenly widens and extends to a little beyond b. Spectra Nos. 5, 6, and 7 illustrate this change. After the decrease of absorption as represented by No. 7, we have the normal increase with elevation of temperature; but still at  $220^\circ \text{C}$ . there is not so much light absorbed as there was at  $16^\circ \text{C}$ . Now the heat or kinetic energy of a body increases with elevation of temperature; therefore the deep orange  $\text{HgI}_2$  at  $220^\circ \text{C}$ . has more kinetic energy than the red modification at  $16^\circ \text{C}$ .; but the latter has greater structural absorption. The rule is that increase of kinetic energy is accompanied by increased structural absorp-

\* Compiled from "Some Experiments on the Density of Garnet, &c." (Church), Chem. Soc. Journ. vol. xvii. pp. 386 & 415.

tion. In the behaviour of mercuric iodide we have the opposite of this, viz. decrease of absorption with increase of kinetic energy, whence we infer that the change of absorption which characterizes metachromatism is not necessarily accompanied by change of kinetic energy.

II. Expansion by heat (*i. e.* decrease of density) is an all but universal law so far as we at present know. There are several exceptions, however; and many of these are among the silicates. Their anomalous behaviour is, as a rule, pointed out by the colour-change, as in the case of the zircon. But there may be change of colour, as in the beryl, without alteration of density, *i. e.* without appreciable molecular approach or recession. On the other hand, we have in olivine an example of change of density (molecular recession) without a corresponding alteration of colour. More facts of the same nature might readily be adduced, from which we infer that molar expansion or contraction is not a necessary concomitant of alteration of structural absorption. We have now excluded from our list of possible concomitants i. and ii $\alpha$ . Hence it would appear that ii $\beta$  is the only necessary concomitant—a conclusion which receives strength when we consider that the gradual increase of structural absorption in such bodies as  $\text{HgO}$ ,  $\text{PtCl}_4$ , and  $\text{Cr}_2\text{O}_5\text{Ko}_2$  terminates with their resolution into lower compounds, or into their constituent elements.

*Relation of Colour to Density.*—An attempt was made in 1765 by Delaval to show the connexion between colour and density. In a paper read before the Royal Society, he observes\* :—"It appears from Newton's experiments that denser substances ought by their greater reflective power, in like circumstances, to reflect the less refrangible rays, and that substances of less density should reflect rays proportionably more refrangible and thereby appear of several colours in the order of their density." The seeming success of this attempt is perhaps due to the comparative nature of his facts; for he compares coloured glasses of each of the following metals in the order of their densities:—gold, red; lead, orange; silver, yellow; copper, green; iron, blue. We can readily see now the error of such a comparison; for the colour of glass must evidently depend upon the state of the metal in it; *e. g.* differently coloured glasses may be obtained from the different oxides of the same metal. To take an instance, whilst the cupric oxide gives a bluish-green glass, we know that the cuprous oxide gives a red one. Nevertheless it will be obvious from our foregoing observations that it is possible to establish a con-

\* Philosophical Transactions, vol. lv. p. 10.

nexion between density and colour as due to structural absorption. Not, however, until we have made out a special relation can we well proceed with certainty to establish a comparative one; nor will this special relation accord with Newton's prediction.

From the nature of metachromatism, it is probable that increase or decrease of interatomic distance will in most cases be accompanied by a corresponding alteration of molecular inter-spaces. Hence in allotropes we shall look for exactly the opposite of what Delaval sought to establish, viz. for cold colours with great density and warm ones with low density.

Metachromatic scale.	Carbon.	Sulphur.	Topaz.	Titanic oxide.	Zircon.	Argentio iodide.	Calcic fluoride.
Black .....	Graphite, 2.35						
Brown .....	.....	Ductile, 1.957	.....	Brookite, 4.085	Brown, 4.515		
Red .....	.....	.....	Pink, 3.533	.....	.....	Dark red, 5.406	
Orange .....							
Yellow .....	.....	Octahedral, 2.05	Yellow, 3.539	Rutile, 4.283	.....	Yellowish green, 5.681.	
Green.....							
Blue .....	.....	.....	.....	.....	.....	.....	Blue, 3.170
White or Colourless }	Diamond, 3.33	.....	.....	.....	White, 4.540	.....	White, 3.176

The fact of phosphorus being an exception does not militate against this relation, since it is highly probable that the other anomalies of this body will receive elucidation when studied from the metachromatic standpoint. The change in colour of mercuric iodide, which we have already referred to, indicates atomic approach; and to ascertain whether this is accompanied by a corresponding molecular motion and consequent change of density has been the object of a large number of experiments. From the peculiar difficulties encountered, however, as yet I have obtained no trustworthy results.

The next step from the special relation we have pointed out to one of a comparative nature would seem to be in an extension of the following law of colour-sequence:—"In a series of anhydrous binary compounds of the same two elements, those which have the highest amount of the basylous element exhibit the least structural absorption; and, *vice versa*, those which have the least amount of the basylous element have the most structural absorption"\*.

\* Chemical News, vol. xxxiv. p. 76.



*The Five Orders of Spectra*.—In the borderland between chemistry and physics, data are being accumulated which tend to make the former a dynamical science. Not the least important of these data are those which furnish us with the connexion between the different kinds of spectra. Professor Roscoe is of opinion\* that there is not sufficient evidence as yet to support the sequence in the five orders proposed by Lockyer† and now generally received; and in the light of metachromatism it may be necessary to modify this sequence. The juxtaposition of the orders constituting a class of spectra we take to be founded upon that merging of the optical characters of one order into those of another which is exhibited when the molecular changes in the radiating or absorbing body cause such optical alterations. Now confining our attention to low-temperature or absorption spectra, it is evident from what has already been said that change in structural absorption alone is available for forming a sequence of orders. Hence we consider the fifth order, as at present received, misplaced, the *unique continuous absorption* being from the nature of the experiment a transverse absorptional effect.

A sequence of orders will readily suggest itself if we keep in mind that change in absorption to which metachromatism is due. Conceive of a colourless body capable of undergoing the whole series of change. It first reflects or transmits rays of every refrangibility; as absorption increases, the band of transmitted or reflected light gradually narrows and becomes less intense; finally the field is darkened and we get continuous absorption. What we propose, then, is shown in the following comparison, made in order that the student may have a clearer conception of the matter.

Now received.		What we propose.	
1st order . . . .	Line spectra . . . . .	Class I.	Radiative.
2nd order . . . .	Channelled-space spectra }		
		Class II.	
		Absorptive.	
3rd order . . . .	Continuous absorption at the blue end . . . . .	Class II.	1. Continuous absorption. 2. Partial absorption, <i>i. e.</i> absorption at the blue and red ends, or at any intermediate part of the spectrum. 3. Continuous spectrum.
4th order . . . .	Continuous absorption at the red end . . . . .		
5th order . . . .	Unique continuous absorp- tion . . . . .		

Class II. 1, 2, and 3 constitute the metachromatic scale. To illustrate the bridging over of 1 and 2 no simpler experi-

\* Watts's Dictionary, 2nd Supplement, p. 1093.

† Proceedings of the Royal Society, June 11, 1874.

ment could be tried than the following. Take a piece of copper wire and cover with suboxide by gently heating over a Bunsen burner. Fuse the covered wire into a capillary glass tube. A scarlet glass is thus obtained which is black whilst hot, *i. e.* exhibits continuous absorption (1), and scarlet whilst cold, exhibiting partial absorption (2). 2 and 3 are bridged over by a host of bodies like  $\text{ZnO}$ . When hot they, as a rule, exhibit absorption at the blue end of the spectrum (2), but when cold they are white or colourless (3).

Respecting the merging of Class I. into Class II. nothing much at present can be said, since questions are involved which require further investigation.

### LIII. *On the Resistance of Fluids.*

By LORD RAYLEIGH, *F.R.S.\**

[Plate V.]

**T**HERE is no part of hydrodynamics more perplexing to the student than that which treats of the resistance of fluids. According to one school of writers, a body exposed to a stream of perfect fluid would experience no resultant force at all, any augmentation of pressure on its face due to the stream being compensated by equal and opposite pressures on its rear. And indeed it is a rigorous consequence of the usual hypotheses of perfect fluidity and of the continuity of the motion, that the resultant of the fluid pressures reduces to a *couple* tending to turn the broader face of the body towards the stream. On the other hand, it is well known that in practice an obstacle does experience a force tending to carry it down stream, and of magnitude too great to be the direct effect of friction; while in many of the treatises calculations of resistances are given leading to results depending on the inertia of the fluid without any reference to friction.

It was Helmholtz who first pointed out that there is nothing in the nature of a perfect fluid to forbid a finite slipping between contiguous layers, and that the possibility of such an occurrence is not taken into account in the common mathematical theory, which makes the fluid flow according to the same laws as determine the motion of electricity in uniform conductors. Moreover the electrical law of flow (as it may be called for brevity) would make the velocity infinite at every sharp edge encountered by the fluid; and this would require a negative pressure of infinite magnitude. It is no answer to

\* Communicated by the Author, having been communicated in substance to the British Association at Glasgow.

this objection that a mathematically sharp edge is an impossibility, inasmuch as the electrical law of flow would require negative pressure in cases where the edge is not perfectly sharp, as may be readily proved from the theory of the simple circular vortex, in which the velocity varies inversely as the distance from the axis.

The application of these ideas to the problem of the resistance of a stream to a plane lamina immersed transversely amounts to a justification of the older theory as at least approximately correct. Behind the lamina the fluid is at rest under a pressure equal to that which prevails at a distance, the region of rest being bounded by a surface of separation or discontinuity which joins the lamina tangentially, and is determined mathematically by the condition of constant pressure. On the anterior surface of the lamina there is an augmentation of pressure corresponding to the loss of velocity.

The relation between the velocity and pressure in a steady stream of incompressible fluid may be obtained immediately by considering the transference of energy along an imaginary tube bounded by stream-lines. In consequence of the steadiness of the motion, there must be the same amount of energy transferred in a given time across any one section of the tube as across any other. Now if  $p$  and  $v$  be the pressure and velocity respectively at any point, and  $\rho$  be the density of the fluid, the energy corresponding to the passage of the unit of volume is  $p + \frac{1}{2}\rho v^2$ , of which the first term represents potential, and the second kinetic energy; and thus  $p + \frac{1}{2}\rho v^2$  must retain the same value at all points of the same stream-line. It is further true, though not required for our present purpose or to be proved so simply, that  $p + \frac{1}{2}\rho v^2$  retains a constant value not merely on the same stream-line, but also when we pass from one stream-line to another, provided that the fluid flows throughout the region considered in accordance with the electrical law.

If  $u$  be the velocity of the stream, the increment of pressure due to the loss of velocity is  $\frac{1}{2}\rho u^2 - \frac{1}{2}\rho v^2$ , and can never exceed  $\frac{1}{2}\rho u^2$ , which value corresponds to a place of rest where the whole of the energy, originally kinetic, has become potential. The old theory of resistances went on the assumption that the velocity of the stream was destroyed over the whole of the anterior face of the lamina, and therefore led to the conclusion that the resistance amounted to  $\frac{1}{2}\rho u^2$  for each unit of area exposed. It is evident at once that this is an overestimate, since it is only near the middle of the anterior face that the fluid is approximately at rest; towards the edge of the lamina the fluid moves outwards with no inconsi-

derable velocity, and at the edge itself retains the full velocity of the original stream. Nevertheless the amount of error involved in the theory referred to is not great, as appears from the result of Kirchhoff's calculation of the case of two dimensions, from which it follows that the resistance per unit of area is  $\frac{\pi}{4+\pi}\rho u^2$  instead of  $\frac{1}{2}\rho u^2$ .

It is worthy of notice that by a slight modification of the conditions of the problem the estimate  $\frac{1}{2}\rho u^2$  may be made accurate. For this purpose the lamina is replaced by the bottom of a box-shaped vessel, whose sides project in the direction from which the stream is flowing, and are sufficiently extended to cause approximate quiescence over the whole of the bottom (Plate V. fig. 1). In the absence of friction, the sides themselves do not contribute any thing to the resistance. It appears from this argument that the increase of resistance due to concavity can never exceed a very moderate value.

Although not very closely connected with the principal subject of this communication, it may be well to state the corresponding result in the case of a compressible fluid such as air. If  $p_0$  be the normal pressure in the stream,  $a$  the velocity of sound corresponding to the general temperature,  $\gamma$  the ratio of the two specific heats,  $\frac{1}{2}\rho u^2$  is replaced by

$$p_0 \left\{ \left( 1 + \frac{\gamma-1}{2} \frac{u^2}{a^2} \right)^{\frac{\gamma}{\gamma-1}} - 1 \right\},$$

which gives the resistance per unit of area. The compression is supposed (as in the theory of sound) to take place without loss of heat; and the numerical value of  $\gamma$  is 1.408.

When  $u$  is small in comparison with  $a$ , the resistance follows the same law as if the fluid were incompressible; but in the case of greater velocities the resistance increases more rapidly. The resistance to a meteor moving at speeds comparable with 20 miles per second must be enormous, as also the rise of temperature due to compression of the air. In fact it seems quite unnecessary to appeal to friction in order to explain the phenomena of light and heat attending the entrance of a meteor into the earth's atmosphere.

But although the old theory of resistance was not very wide of the mark in its application to the case of a lamina against which a stream impinges directly, the same cannot be said of the way in which the influence of obliquity was estimated. It was argued that inasmuch as a lamina moved edgeways through still fluid would create no disturbance (in the absence of friction), such an edgeways motion would produce no alteration in the resistance due to a stream perpendi-

cular to the plane of the lamina ; and from this it would follow that when a lamina is exposed to an oblique stream, the resistance experienced would be that calculated from the same formula as before, on the understanding that  $u$  now represents the *perpendicular component* of the actual velocity of the stream. Or if the actual velocity of the stream be  $V$ , and  $\alpha$  denote the angle between the direction of the stream and the lamina, the resistance would be per unit of area

$$\frac{1}{2}\rho V^2 \sin^2 \alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This force acts of course perpendicularly to the plane of the lamina ; the component down the current is

$$\frac{1}{2}\rho V^2 \sin^3 \alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The argument by which this result is obtained, however, is quite worthless ; and the law of the squares of the sines expressed in (1) is known to practical men to be very wide of the mark, especially for small values of  $\alpha$ . The resistance at high obliquities is much greater than (1) would make it, being more nearly in proportion to the first power of  $\sin \alpha$  than to the square.

As a proof that an edgeways motion of an elongated body through water is not without influence on the force necessary to move it with a given speed broadways, Mr. Froude says\*, “ Thus when a vessel was working to windward, immediately after she had tacked and before she had gathered headway, it was plainly visible, and it was known to every sailor, that her leeway was much more rapid than after she had begun to gather headway. The more rapid her headway became, the slower became the lee-drift, not merely relatively slower, but absolutely slower.”

“ Again, any one might obtain conclusive proof of the existence of this increase of pressure occasioned by the introduction of the edgeways component of motion, who would try the following simple experiment. Let him stand in a boat moving through the water, and, taking an oar in his hand, let him dip the blade vertically into the water alongside the boat, presenting its face normally to the line of the boat’s motion, holding the plane steady in that position, and let him estimate the pressure of the water on the blade by the muscular effort required to overcome it. When he has consciously appreciated this, let him begin to sway the blade edgeways like a pendulum, and he will at once experience a very sensible increase of pressure. And if the edgeways sweep thus as-

\* Proceedings of the Society of Civil Engineers, vol. xxxii., in a discussion on a paper by Sir F. Knowles on the Screw Propeller.

signed to the blade is considerable and is performed rapidly, the greatness of the increase in the pressure will be astonishing until its true meaning has been realized. Utilizing this proposition, many boatmen, when rowing a heavy boat with narrow-bladed oars, were in the habit of alternately raising and lowering the hand with a reciprocating motion, so as to give an oscillatory dip to the blade during each stroke, and thus obtained an equally vigorous reaction from the water with a greatly reduced slip or sternward motion of the blade."

It is not difficult to see that in the case of obliquity we have to do with the whole velocity of the current, and not merely with the resolved part. Behind the lamina there must be a region of dead water bounded by a surface of discontinuity, within which the pressure is the same as if there were no obstacle. On the front face of the lamina there must be an augmentation of pressure, vanishing at the edges and increasing inwards to a maximum at the point where the stream divides. At this point the pressure is  $\frac{1}{2}\rho V^2$ , corresponding to the loss of the *whole* velocity of the stream. It is true that the maximum pressure prevails over only an infinitely small fraction of the area; but the same may be said even when the incidence of the stream is perpendicular.

The exact solution of the problem in the case of two dimensions which covers almost all the points of practical interest, can be obtained by the analytical method of Helmholtz and Kirchhoff\*. If an elongated blade be held vertically in a horizontal stream, so that the angle between the plane of the blade and the stream is  $\alpha$ , the mean pressure is

$$\frac{\pi \sin \alpha}{4 + \pi \sin \alpha} \rho V^2, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

varying, when  $\alpha$  is small, as  $\sin \alpha$ , and not as  $\sin^2 \alpha$ . The proof will be found at the end of the present paper.

The fact that the resistance to the broadways motion of a lamina through still fluid can be increased enormously by the superposition of an edgeways motion is of great interest. For example, it will be found to be of vital importance in the problem of artificial flight.

According to the old theory the component of resistance transverse to the stream varied as  $\sin^2 \alpha \cos \alpha$ , and attained its maximum for  $\alpha = 55^\circ$  nearly. The substitution of expression

\* Formulæ (3) and (4) were given at the Glasgow Meeting of the British Association. I was then only acquainted with Kirchhoff's "Vorlesungen über mathematische Physik," and was not aware that the case of an oblique stream had been considered by him (*Crelle*, Bd. 70, 1869). However, Kirchhoff has not calculated the forces; so that the formulæ are new.

(3) for  $\sin^2 \alpha$  will materially modify the angle at which the transverse force is greatest. The quantity to be made a maximum is

$$\frac{\sin \alpha \cos \alpha}{4 + \pi \sin^2 \alpha};$$

and the value of  $\alpha$  for which the maximum is attained is  $\alpha = 39^\circ$  nearly, being considerably less than according to the old theory, on account of the increased value of the normal pressure at high obliquities.

The pressure, whose mean amount is given in (3), is far from symmetricaly distributed over the breadth of the blade, as might be anticipated from the fact that the region of maximum pressure, where the stream divides, is evidently nearer to the anterior or up-stream edge. If the breadth of the blade be called  $l$ , the distance ( $x$ ) of the centre of pressure, reckoned from the middle, is

$$x = \frac{3}{4} \cdot \frac{\cos \alpha \cdot l}{4 + \pi \sin^2 \alpha} \cdot \cdot \cdot \cdot \cdot \cdot (4)$$

If the blade be pivoted so as to be free to turn about an axis parallel to its edges, (4) gives the position of the axis corresponding to any angle of inclination  $\alpha$ . If  $\alpha = 90^\circ$ ,  $x = 0$ , as is evident from symmetry. As  $\alpha$  diminishes, the corresponding value of  $x$  increases and reaches a maximum, viz.  $\frac{3}{16} l$ , when  $\alpha = 0$ . The axis then divides the breadth of the blade in the ratio 11 : 5.

These results may be stated in another form as follows. If the axis of suspension divide the width in a more extreme ratio than 11 : 5, there is but one position of stable equilibrium, that namely in which the blade is parallel to the stream with the narrower portion directed upwards. If the axis be situated exactly at the point which divides the width in the ratio 11 : 5, this position becomes neutral, in the sense that for small displacements the force of restitution is of the second order, but the equilibrium is really stable. When the axis is still nearer the centre of figure, the position parallel to the stream becomes unstable, and is replaced by two inclined positions given by (4), making with the stream equal angles, which increase from zero to a right angle as the axis moves in towards the centre. With the centre line itself for axis, the lamina can only remain at rest when transverse to the stream, though of course with either face turned upwards.

The fact, rather paradoxical to the uninitiated, that a blade free to turn about its centre line sets itself transversely, may be

easily proved by experiment. For this purpose it is sufficient to take a piece of thin brass plate shaped as in the figure (Plate V. fig. 2), and mount it with its points bearing in two small indentations in a U-shaped strip of thicker plate, easily made by striking the strip with a conically pointed piece of steel driven by the hammer. When this little apparatus is moved through the water, the movable piece at once sets itself across the direction of motion. The same result may be observed when the apparatus is exposed to the wind; but in this case an unexpected phenomenon often masks the stability of the transverse position. It is found that when the plate is set rotating, the force of the wind will maintain or accelerate the motion. This effect might be supposed to be due to a want of symmetry, were it not that the rotation occurs in either direction. It is evidently connected with the disturbance of the fluid due to the motion of rotation, and is not covered by the calculation leading to formula (4), which refers to the forces experienced when the blade is *at rest* in any position.

I am not aware of any experimental measurements with which (4) could be compared; but the result that the equilibrium parallel to the stream is indifferent when the axis is situated in the position defined by the ratio 11 : 5, is in agreement with the construction of balanced rudders, of which the front part is usually made of about one half the width of the hinder part.

The accompanying Table contains some numerical examples of the general formulæ. The first column gives the angle between the lamina and stream, the second the value of  $\sin^2 \alpha$ , to which, on the old theory, the resistance should be proportional; the third column is derived from some experiments by Vince on water, published in the 'Philosophical Transactions' for 1798.

$\alpha$ .	$\sin^2 \alpha$ .	Vince.	$\frac{\sin \alpha(4+\pi)}{4+\pi \sin \alpha}$ .	$\frac{3}{4} \frac{\cos \alpha}{4+\pi \sin \alpha}$ .	
90	1.0000	1.000	1.0000	.0000	.5000
70	.8830	.974	.9652	.0369	.2676
50	.5868	.873	.8537	.0752	.0981
30	.2500	.663	.6411	.1166	.0173
20	.1170	.458	.4814	.1389	.0040
10	.0302	.278	.2728	.1625	.0004

The quantity directly measured by Vince was the resolved part of the resistance in the direction of the stream, from which the tabulated number is derived by division by  $\sin \alpha$ . The fourth column represents the law of resistance according to the formula now proposed, a factor being introduced so as to make



the maximum value unity. The fifth column gives the distance between the centre of pressure and the middle line of the blade, expressed as a fraction of the total width. The sixth column is the value of

$$\frac{2(1 - 2 \cos \alpha + \cos^3 \alpha) + \alpha \sin \alpha}{4 + \pi \sin \alpha},$$

which is the distance from the anterior edge of the point where the stream divides, and where accordingly the pressure attains its greatest value. It will be seen that, as might be expected, this distance becomes small at moderate obliquities.

The result of Vince's experiments agrees with theory remarkably well; and the contrast with  $\sin^2 \alpha$  is especially worthy of note. The experiments were made with a whirling machine, and appear to have been carefully conducted; but they were on too small a scale to be quite satisfactory. The subject might now be resumed with advantage.

From theory it would appear that any part of the region of dead water behind the lamina might be filled up with solid matter without in any way disturbing the motion or altering the resistance; but in practice with actual fluids this statement must not be taken without qualification. If the boundary of the solid approach too nearly the natural position of the surface of separation, the intervening fluid appears to be sucked out until the lines of flow follow the surface of the obstacle. This is the state of things aimed at, and approximately attained, in well-designed ships, round which the water flows nearly according to the electrical law. The resistance is then of an entirely altered character, and depends only upon the friction against the skin.

It was observed by Sir William Thomson at Glasgow, that motions involving a surface of separation are unstable. This is no doubt the case, and is true even of a parallel jet moving with uniform velocity. If from any cause a slight swelling occurs at any point of the surface, an *increase* of pressure ensues tending not to correct but to augment the irregularity. I had occasion myself to refer to a case of this kind in a paper on Waves, published in the 'Philosophical Magazine' for April 1876. But it may be doubted whether the calculations of resistance are materially affected by this circumstance, as the pressures experienced must be nearly independent of what happens at some distance in the rear of the obstacle, where the instability would first begin to manifest itself.

The formulæ proposed in the present paper are also liable to a certain amount of modification from friction which it would be difficult to estimate beforehand, but which cannot be

very considerable, if the experiments of Vince are to be at all relied on.

In the following analysis  $\phi$  and  $\psi$  are the potential and stream functions,  $z = x + iy$ ,  $\omega = \phi + i\psi$ ; and it is known that the general conditions of fluid motion in two dimensions are satisfied by taking  $z$  as an arbitrary function of  $\omega$ . If

$$\frac{dz}{d\omega} = \zeta = \rho (\cos \theta + i \sin \theta), \quad . . . \quad (A)$$

Kirchhoff shows that  $\zeta$  represents the velocity of the stream at any point, with the exception that its modulus  $\rho$  is proportional to the reciprocal of the velocity instead of to the velocity itself. If the general velocity of the stream be unity, the condition to be satisfied along a surface of separation bounding a region of dead water is  $\rho = 1$ . The value of  $\psi$  must of course also preserve a constant value along the same surface.

The form of  $\zeta$  applicable to the present problem is

$$\zeta = \cos \alpha + \frac{1}{\sqrt{\omega}} + \sqrt{\left(\cos \alpha + \frac{1}{\sqrt{\omega}}\right)^2 - 1}. \quad (B)$$

When  $\omega = \infty$ ,

$$\zeta = \cos \alpha - i \sin \alpha.$$

The surface of separation corresponds to  $\psi = 0$ , for which value of  $\psi$   $\omega$  becomes real; and the point at which the stream divides corresponds to  $\omega = 0$ , for which  $\zeta = \infty$ . For  $\psi = 0$  and real values of  $\cos \alpha + \frac{1}{\sqrt{\omega}}$  less than unity,  $\rho = 1$ . This portion therefore corresponds to the surface of separation, for which the pressure is constant. When  $\cos \alpha + \frac{1}{\sqrt{\omega}}$  is real and greater than unity,  $\zeta$  is real, indicating that the direction of motion is parallel to the axis of  $x$ . This part corresponds to the anterior face of the lamina.

The augmentation of pressure at any point is represented by  $\frac{1}{2} \left(1 - \frac{1}{\rho^2}\right)$ , if the density of the fluid be taken as unity; and thus the whole resistance is measured by the integral

$$\int \frac{1}{2} \left(1 - \frac{1}{\rho^2}\right) dl,$$

if  $dl$  represents an element of the width of the lamina. Kirchhoff shows how to change the variable of integration from  $l$  to  $\omega$ . The velocity of the fluid is  $\frac{d\phi}{dl}$ , or, since  $\psi$  is here zero,

$\frac{d\omega}{dl}$ . Thus, since  $\zeta$  is real,  $\pm\zeta=\rho=\frac{dl}{d\omega}$ ; and therefore the integral may be replaced by

$$\int \pm \frac{1}{2} \left( \zeta - \frac{1}{\zeta} \right) d\omega, \quad . \quad . \quad . \quad . \quad . \quad (C)$$

in which all the elements are to be taken positive.

From the form of  $\zeta$  in (B), it appears that

$$\frac{1}{2}\left(\xi - \frac{1}{\xi}\right) = \sqrt{\left(\cos \alpha + \frac{1}{\sqrt{\omega}}\right)^2 - 1}. \quad (\text{D})$$

The width of the lamina  $l$  is  $\int \zeta d\omega$ , where the limits of integration are such as make

$$\cos \alpha + \frac{1}{\sqrt{\omega}} = \pm 1.$$

The integration may be effected by the introduction of a new variable  $\beta$ , where

$$\beta = \sin^2 \alpha \sqrt{\omega - \cos \alpha},$$

and the limits for  $\beta$  are  $\pm 1$ . Thus

$$\int \left( \cos \alpha + \frac{1}{\sqrt{\omega}} \right) d\omega = \frac{\beta^2 \cos \alpha}{\sin^4 \alpha} + \frac{2\beta}{\sin^4 \alpha} + \text{const.};$$

and therefore between the limits  $\pm 1$  we have

$$4 \div \sin^4 \alpha.$$

The second part of  $\zeta$  may be written  $\sqrt{1-\beta^2} \div \sin \alpha \sqrt{\omega}$ , giving the integral

$$\int \frac{d\omega}{\sin \alpha \sqrt{\omega}} \sqrt{1-\beta^2} = \frac{\beta \sqrt{1-\beta^2}}{\sin^3 \alpha} + \frac{\sin^{-1} \beta}{\sin^3 \alpha} + \text{const.}$$

Thus the complete value of  $z$  between the limits, or  $l$ , is

$$l = \frac{4}{\sin^4 \alpha} + \frac{\pi}{\sin^3 \alpha} = \frac{4 + \pi \sin \alpha}{\sin^4 \alpha}. \quad \dots \quad (\text{E})$$

By (C) and (D) the whole pressure on the lamina is represented by the second part of  $l$  in (E), or  $\pi \div \sin^3 \alpha$ ; so that the mean pressure is

$$\frac{\pi}{\sin^3 \alpha} \div \frac{4 + \pi \sin \alpha}{\sin^4 \alpha} = \frac{\pi \sin \alpha}{4 + \pi \sin \alpha},$$

as was to be proved.

Again, the elementary moment of pressure about  $z=0$  is

$$\pm \frac{1}{2} \left( \zeta - \frac{1}{\xi} \right) d\omega, z = \frac{2\sqrt{1-\beta^2}}{\sin^3 \alpha} z d\beta.$$

Now if the arbitrary constant be taken suitably, the complete value of  $z$  is

$$z = \frac{\beta^2 \cos \alpha + 2\beta}{\sin^4 \alpha} + \frac{\beta \sqrt{1-\beta^2} + \sin^{-1} \beta}{\sin^3 \alpha}.$$

The odd terms in  $z$  will contribute nothing to the integral; and therefore we may take for the moment of pressure about  $z=0$ ,

$$\int_{-1}^{+1} \frac{2\sqrt{1-\beta^2}}{\sin^3 \alpha} \cdot \frac{\beta^2 \cos \alpha}{\sin^4 \alpha} d\beta = \frac{\pi}{\sin^3 \alpha} \cdot \frac{\cos \alpha}{4 \sin^4 \alpha}.$$

In this result the first factor represents the total pressure, and therefore  $\frac{\cos \alpha}{4 \sin^4 \alpha}$  expresses the distance of the centre of pressure from the point  $z=0$ . With the same origin the value of  $z$  for the middle of the lamina is  $\frac{\cos \alpha}{\sin^4 \alpha}$ ; and thus the displacement of the centre of pressure from the middle of the lamina is

$$-\frac{3}{4} \frac{\cos \alpha}{\sin^4 \alpha}.$$

This distance must now be expressed in terms of  $l$  or

$$4 + \pi \sin \alpha \div \sin^3 \alpha,$$

which gives as the final result,

$$-\frac{3}{4} \frac{\cos \alpha \cdot l}{4 + \pi \sin \alpha}.$$

The negative sign indicates that the centre of pressure is on the upstream side of the middle point.

As to the form of the surface of separation, its intrinsic equation is given at once by the value of  $\zeta$  in terms of  $\omega$ . The real part of  $\zeta$  is  $\cos \theta$  (since  $\rho=1$ ), where  $\theta$  is the angle between the tangent at any point and the plane of the lamina. Along the surface of separation  $\omega$  is identical with  $\phi$ , and  $\frac{d\phi}{ds}=1$ . Thus if  $s$  be the length of the arc of either branch measured from the point where it joins the lamina, the intrinsic equation is

$$\cos \theta = \cos \alpha \pm \frac{1}{\sqrt{s+c}},$$

and the constant is to be determined by the condition that  $s=0$  when  $\cos \theta = \pm 1$ . Since  $\cos \theta = \frac{dx}{ds}$ , the relation between  $x$  and  $s$  is readily obtained on integration; but the relation between  $y$  and  $s$  is more complicated.

In the case of perpendicular incidence  $\cos \alpha = 0$ ,  $c = 1$ , so that

$$\cos \theta = \pm \frac{1}{\sqrt{s+1}},$$

giving on integration

$$x = 2\sqrt{s+1} + \text{const.}$$

It appears that the value of  $x$  does not approach a finite limit as  $s$  increases indefinitely.

#### LIV. *Notes on Hydrodynamics*.

By LORD RAYLEIGH, F.R.S.\*

[Plate V.]

##### *The Contracted Vein.*

THE contraction of a jet of fluid in escaping from a higher to a lower pressure through a hole in a thin plate has been the subject of much controversy. Of late years it has been placed in a much clearer light by a direct application of the principle of momentum to the circumstances of the problem by Messrs. Hanlon and Maxwell † among others.

For the sake of simplicity the liquid will be supposed to be unacted upon by gravity, and to be expelled from the vessel by the force of compressed air through a hole of area  $\sigma$  in a thin plane plate forming part of the sides of the vessel. After passing the hole the jet contracts, and at a little distance assumes the form of a cylindrical bar of reduced area  $\sigma'$ . The ratio  $\sigma' : \sigma$  is called the coefficient of contraction.

The velocity acquired by the fluid in escaping from the pressure  $p$  is determined, in the absence of friction, by the principle of energy alone. If the density of the fluid be unity, and the acquired velocity  $v$ ,

$$v^2 = 2p. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The product of  $v$ , as given by (1), and  $\sigma$  is sometimes, though very improperly, called the theoretical discharge; and it differs from the true discharge for two reasons. In the first place, the velocity of the fluid is not equal to  $v$  over the whole of the area of the orifice. At the edge, where the jet is free, the velocity is indeed  $v$ ; but in the interior of the jet the pressure is above atmosphere, and therefore the velocity less than  $v$ . And, secondly, it is evident that the quantity of fluid passing the orifice depends, not upon the whole velocity with

\* Communicated by the Author.

† Proceedings of the Mathematical Society, November 11, 1869.

which the fluid may be moving at any point, but upon the resolved part of this velocity in a direction perpendicular to the plane of the orifice. Thus it is only in the middle of the jet that the whole velocity is efficient; near the edge the motion is tangential; and consequently this part contributes but little to the discharge. It is certain that the discharge will be considerably *less* than  $v\sigma$ , or, which is the same thing, that the jet must undergo considerable contraction before the liquid composing it can move in parallel lines with uniform velocity  $v$ .

Since the actual discharge is  $\sigma'v$ , the quantity of momentum passing away with the jet in unit time is  $\sigma'v^2$ , and the force generating this momentum is that necessary to hold the vessel at rest. If the whole of the interior surface of the vessel were subject to the pressure  $p$ , this force would have no existence. On account of the orifice the equilibrium of internal pressures is disturbed and a force  $p\sigma$  is uncompensated. But this is not all. Not only is the pressure that would have acted over the area of the orifice wanting, but there is also a relief of pressure on the surface surrounding the orifice corresponding to the velocity with which the fluid there moves. The uncompensated force tending to produce recoil may therefore be represented by  $(\sigma + \delta\sigma)p$ , where  $\delta\sigma$  is a small positive quantity; and if the vessel is to remain at rest, a force of this magnitude must be applied to it acting in the direction in which the jet escapes. Thus

$$(\sigma + \delta\sigma)p = \sigma'v^2; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and therefore, by (1),  $\sigma' = \frac{1}{2}(\sigma + \delta\sigma), \quad . \quad . \quad . \quad . \quad . \quad (3)$

expressing that the coefficient of contraction is greater than  $\frac{1}{2}$ .

In the absence of a mathematical solution of the problem it is impossible to estimate the magnitude of  $\delta\sigma$  with any precision; but it is something to know from general principles that there must be a considerable contraction, and yet that the coefficient of contraction must exceed one half. However, by a slight modification of the problem it is possible to get rid of the uncertainty arising from the unknown magnitude of  $\delta\sigma$ . Suppose the hole in a thin plate to be replaced by a thin parallel tube projecting into the interior of the vessel. If the tube be long enough, the sides of the vessel are sufficiently removed from the region of rapid flow to allow of the pressure acting upon them being treated as constant, while the relief of pressure on the sides of the *tube* does not add any thing to the forces tending to produce momentum in the jet. Under these circumstances, if  $\sigma$  be the area of the section of the tube and  $\sigma'$  the area of the section of the jet after contraction,  $\sigma' = \frac{1}{2}\sigma$ , or the coefficient of contraction is one half exactly. The rigorous mathematical solution of this problem, so far as relates to

the case of motion in two dimensions, has been given by Helmholtz (Phil. Mag. November 1868); and the conclusion that the width of the emergent stream is ultimately one half that of the channel follows from his analysis\*.

This problem throws some light on the formation of a surface of discontinuity. If the electrical law of flow held good so that the tube were filled, twice as much momentum as before would have to be generated, and the extra momentum would have its origin in the infinite negative pressure which, according to that law, must prevail over the extreme edge of the tube. In the absence of forces capable of generating the extra momentum the tube could not flow full.

A generalization of the problem just considered may be effected by replacing the vessel, whose dimensions were supposed to be indefinitely great, by a cylinder of finite section  $\sigma''$  (Plate V. fig. 3), in which the fluid moves with finite velocity  $v''$ . If  $v'$  and  $\sigma'$  be the ultimate velocity and section of the escaping jet, the equation of continuity gives

$$v'\sigma' = v''\sigma'' \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

By the principle of energy,

$$p = \frac{1}{2}(v'^2 - v''^2); \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and by the principle of momentum, if  $\sigma$  be the area of the tube,

$$p\sigma = \sigma'v'^2 - \sigma''v''^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From these equations we obtain

$$\frac{2}{\sigma} = \frac{1}{\sigma'} + \frac{1}{\sigma''}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

showing that the section of the tube is an harmonic mean between the sections of the cylinder and of the jet.

The problem of the contracted vein for a hole in a thin plate has been solved mathematically by Kirchhoff† for the case of motion in two dimensions. As this solution is very little known, and many points of interest are passed over by Kirchhoff himself, a short account of it accompanied by a few remarks and calculations may not be out of place.

With the notation explained in the previous paper, the form of  $\zeta$  proper to this problem is

$$\zeta = e^{-\omega} + \sqrt{e^{-2\omega} - 1}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

\* The application of the principle of momentum to the case of the inverted tube was original with myself, but, as I learned at Glasgow, had been made previously by Mr. Froude. In small-scale experiments the result is liable to be vitiated by adhesion to the sides of the tube.

† *Crelle*, vol. lxx. 1868, and *Vorlesungen über mathematische Physik*.

The values of  $\psi$  corresponding to the boundaries of the jet are 0 and  $\pi$ ; and the stream-line which passes symmetrically through the middle of the orifice is  $\psi = \frac{1}{2}\pi$ , for which value of  $\psi$   $\zeta$  is purely imaginary. For the stream-line  $\psi = 0$ ,

$$\zeta = e^{-\phi} + \sqrt{e^{-2\phi} - 1}. \quad \dots \quad (9)$$

When  $\phi$  is negative in (9)  $\zeta$  is wholly real and positive, so that this part of the stream-line is parallel to the axis of  $x$ , and answers to the bottom of the vessel up to the edge of the orifice. When  $\phi$  is positive  $\zeta$  is complex, but its modulus is unity. This part therefore corresponds to the free boundary.

The width of the jet after contraction is  $\pi$ , since the velocity is unity; and the total flow between the stream-lines  $\psi = 0$  and  $\psi = \pi$  is measured by the difference of the values of  $\psi$ .

In equation (9) the real part of  $\zeta$  ( $\phi$  positive) is  $\cos \theta$ , where  $\theta$  is the angle between the direction of motion at any point and the axis of  $x$ ; so that the intrinsic equation to the boundary is

$$\cos \theta = \frac{dx}{ds} = e^{-s}, \quad \dots \quad (10)$$

no constant being added if  $s$  be measured from the edge of the orifice where  $\cos \theta = 1$ .

From (10), by integration,

$$x = 1 - e^{-s}, \quad \dots \quad (11)$$

if the origin of  $x$  be taken at the edge of the orifice, where  $s = 0$ . This equation determines the width of the aperture. When  $s = \infty$ ,  $x = 1$ , which corresponds to the abscissa of the boundary of the jet after contraction; and, as we have already seen, the width of the jet itself is  $\pi$ . Accordingly the whole width of the aperture is  $2 + \pi$ , and the coefficient of contraction  $\pi : 2 + \pi$ .

The numerical value of  $\pi : 2 + \pi$  is  $\cdot 611$ , agreeing very nearly with the coefficient of contraction found by observation.

From (10),

$$\frac{dy}{ds} = \sqrt{1 - e^{-2s}},$$

whence

$$y = \sqrt{1 - e^{-2s}} - \frac{1}{2} \log \frac{1 + \sqrt{1 - e^{-2s}}}{1 - \sqrt{1 - e^{-2s}}}, \quad \dots \quad (12)^*$$

if the origin of  $y$  be taken at  $s = 0$ .

If we eliminate  $s$  between (11) and (12), we get as the equation of the curve in Cartesian coordinates,

$$y = \sqrt{2x - x^2} - \frac{1}{2} \log \frac{1 + \sqrt{2x - x^2}}{1 - \sqrt{2x - x^2}}, \quad \dots \quad (13)$$

\* Equations (11) and (12) are given by Kirchhoff.



from which the following points are calculated:—

$x=.1$	$-y=.0313$	$x=.6$	$-y=.6494$
$x=.2$	$-y=.0932$	$x=.7$	$-y=.9203$
$x=.3$	$-y=.1815$	$x=.8$	$-y=1.3127$
$x=.4$	$-y=.2985$	$x=.9$	$-y=1.9915$
$x=.5$	$-y=.4509$	$x=1.0$	$-y=\infty$

By means of these points the curve, fig. 6, is constructed. From (10),  

$$s = -\log \cos \theta;$$

so that the radius of curvature is  $\tan \theta$ . The curvature is therefore infinite at the origin, and diminishes continually as  $s$  increases.

In discussions on the cause of the contraction of the jet doubts have been expressed as to the reality of the deficiency of velocity in the middle of the orifice; and it may therefore be worth while to examine this point more closely. For this purpose it will be convenient to express  $z$  or  $x + iy$  in terms of  $\zeta$ .

From (8) we get

$$e^{-\omega} = \frac{1 + \zeta^2}{2\zeta}, \quad \dots \dots \dots (14)$$

whence

$$\frac{d\omega}{d\zeta} = \frac{1}{\zeta} \frac{1 - \zeta^2}{1 + \zeta^2} \quad \dots \dots \dots (15)$$

Thus

$$z = \int \zeta d\omega = \int \zeta \frac{d\omega}{d\zeta} d\zeta = \int \frac{1 - \zeta^2}{1 + \zeta^2} d\zeta = 2 \tan^{-1} \zeta - \zeta + C.$$

In order to determine the value of the constant of integration, we may observe that  $\zeta$  when real varies between  $-\infty$  and  $-1$ , and between  $+1$  and  $+\infty$ . The values  $\pm 1$  correspond to the edges of the orifice when  $z=0$  and  $z=\pi+2$ . Hence  $\tan^{-1}\zeta$  varies between  $\frac{\pi}{4}$  and  $\frac{3\pi}{4}$ , and  $C = 1 - \frac{\pi}{2}$ . Accordingly

$$z = 2 \tan^{-1} \zeta - \zeta + 1 - \frac{\pi}{2}. \quad \dots \dots \dots (16)$$

If  $v$  be the velocity of the stream at any point  $y$  of the line of symmetry  $x = \frac{\pi}{2} + 1$ ,  $\zeta = -\frac{i}{v}$ ; and therefore by (16) the relation between  $y$  and  $v$  is

$$iy = -2 \tan^{-1} \frac{i}{v} + \frac{i}{v} - \pi;$$

or, if  $\tan^{-1} \frac{i}{v}$  be replaced by its logarithmic equivalent,

$$y = \frac{1}{v} - \log \frac{1+v}{1-v}. \quad \dots \dots \dots (17)$$

A few pairs of corresponding values of  $y$  and  $v$  will give an idea of the relation expressed in (17).

$v = \frac{9}{10}$	$y = -1.834$
$v = \frac{3}{4}$	$y = -.613$
$v = \frac{2}{3}$	$y = -.1095$
$v = \frac{7}{12}$	$y = +.3792$
$v = \frac{1}{2}$	$y = +.902$
$v = \frac{1}{4}$	$y = +3.489.$

By interpolation we find that, corresponding to  $y=0$ ,  $v=.6840$ ,  $v^2=.420$ . Hence the pressure in the middle of the orifice is .58 of that prevailing in the vessel, the external pressure being treated as zero. In these statements the ultimate velocity is understood to be unity, and the scale of linear magnitude is such that  $2 + \pi$  represents the width of the orifice.

### *Meeting Streams.*

The principle of momentum gives interesting information on the question of the mutual action of streams which come into collision. Suppose, for example, that the motion is in two dimensions, and that two equal streams moving with the same velocity meet at an angle  $2\alpha$  (fig 4). After the collision the fluid resolves itself into two other streams of unequal width parallel to the line bisecting the angle  $2\alpha$ ; and a question arises as to the relative magnitude of these streams. The ultimate velocity is of course in both parts the same as before the collision.

The width of the original streams being unity, let us suppose that the width of that derived stream which is least diverted is  $x$ . The width of the other derived stream is then  $2-x$ ; and the principle of conservation of momentum gives at once the relation

$$2 \cos \alpha = x - (2-x),$$

whence  $x = 1 + \cos \alpha$ . The ratio of the two derived streams is

$$2-x : x = \tan^2 \frac{\alpha}{2}. \quad . \quad . \quad . \quad . \quad (18)$$

For example, if  $\alpha = 60^\circ$ , the ratio of the derived streams is 1:3. The effect of friction would be to make the ratio still more extreme.

If we suppose the motion reversed, we obtain the solution of the problem of the direct impact of two streams of unequal widths which meet with equal velocities. The ratio of streams being known, (18) determines the angle of divergence.

In the case where  $\alpha$  is a right angle the four streams are all equal, and the bounding surfaces are symmetrical with respect to the straight lines bisecting the streams (fig. 5). The exact solution of this case has been indicated by Kirchhoff. If  $\theta$  be the angle between the tangent to the free surface at any point and

the axis of  $x$ , and if  $s$  be the length of the arc measured from the middle point (where  $\theta = \pm \frac{\pi}{4}$ ), the intrinsic equation to the free surface is

$$\tan \theta = \pm e^{-s},$$

the scale of linear magnitude being such that the initial and final width of the streams is  $\pi$ .

*Postscript*, Nov. 14.—Although the accurate determination by experiment of the pressure in the interior of a stream is a matter of considerable difficulty, the theoretical value of the pressure in the middle of a jet issuing through a slit in a thin plate has been recently verified approximately by Mr. A. Mallock in my laboratory. The fraction of the original head still remaining in the plane of the plate was found to be about  $\cdot 59$ , which agrees very closely with the value obtained from theory, viz.  $\cdot 58$ . For a circular hole the corresponding fraction is higher, about  $\cdot 67$ ; but for this there is at present no theory. The observations were made by introducing along the axis of the jet a fine glass tube in the side of which was a small hole, the interior of the tube being in connexion with a manometer.

LV. *Notes on the "Seiches" of the Swiss Lakes.*

By Professor FOREL.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I HAVE received the following notes from Professor Forel concerning the "Seiches" of the Swiss Lakes. Professor Forel was so good last summer as to communicate to the Physical Society of London the results of his observations on these phenonema, which he regards as mononodal oscillations of the water affected by the depths of the lakes. The present notes of Dr. Forel will be of interest to those of your readers who are acquainted with that gentleman's previous observations; and they have a direct bearing upon the study of stationary liquid waves, some experiments on which were published by me in your Magazine.

Your obedient Servant,

FREDERICK GUTHRIE.

(*Professor Forel to Frederick Guthrie*).

My very honoured Colleague, Morges, 29th Sept. 1876.

I have the satisfaction of being able to inform you that I have at last found a formula which is perfectly adapted to the "Seiches" in our lakes, inasmuch as it takes into account their length and depth.

In a pamphlet by Dr. J. Rud. Mérian, of Basle, 'Ueber die Bewegung tropfbarer Flüssigkeiten in Gefässen,' Basel, 1828, which was communicated to me by the grandson of the author, my friend Prof. K. von der Mühle, of Leipzig, I find the formula

$$t = \sqrt{\frac{\pi l}{g}} \left\{ \frac{e^{\frac{\pi h}{l}} + e^{-\frac{\pi h}{l}}}{e^{\frac{\pi h}{l}} - e^{-\frac{\pi h}{l}}} \right\}^{\frac{1}{2}},$$

which expresses in function of the length ( $l$ ) and of the depth ( $h$ ) of the basin, the duration  $t$  of the motion of oscillation;  $t$  is the duration of a half oscillation of what I have called *la seiche haute et la seiche basse*, or, *la seiche ascendante et la seiche descendante*.

This formula is perfectly applicable to our lake *seiches*.

For convenience of calculation I have transformed it into

$$t = \sqrt{\frac{\pi l}{g}} \left\{ \frac{\left( e^{\frac{\pi h}{l}} \right)^2 + 1}{\left( e^{\frac{\pi h}{l}} \right)^2 - 1} \right\}$$

and into

$$h = \frac{l}{\pi} \left\{ \frac{\log \sqrt{\frac{t^2 + \frac{\pi l}{g}}{t^2 - \frac{\pi l}{g}}}}{\log e} \right\};$$

and on applying these formulæ to our lakes I find that they express very sufficiently the relations between the duration of the *seiches* and the dimensions of the lakes.

I assume at starting that the formula which has been calculated for a basin with a horizontal bottom, is applicable to a basin of irregular depth if for  $h$  we adopt the mean depth (Comp. I<sup>re</sup> étude sur les *seiches*, p. 36). Perhaps this point ought to be submitted to a fresh study. However this may be, I append a few examples of the application of my formulæ to the *seiches*.

Transversal seiches of Lake Leman . . .  $2t = 600''$   
 $l = 13800$  metres . . . . .  $t = 300''$

The formula gives  $h = 216$  metres. Now the greatest depth of the lake is 334 metres; and accordingly this value of 216 metres for the mean depth of the large lake is perfectly plausible.

Longitudinal seiches of Lake Leman . .  $2t = 4200''$   
 $l = 73200$  metres . . . . .  $t = 2100''$

The formula gives  $h=130$  metres, a value which I therefore assign as mean depth to the whole of the lake, including the little lake near Geneva. Including this additional lake,  $t=2069''$ .

Longitudinal seiches of Lake Brienzen.

$$l=13700 \quad 2t=574'' \quad t=287''$$

Greatest depth 261 metres.

Mean depth calculated 233 metres, and so on.

In my observations, however, I have to notice two exceptions, which I hope may be accounted for by errors of observation. They relate to the Lakes of Wallenstadt and Zug. As soon as I have time, I shall endeavour to reexamine my experiments on these two lakes. This, dear Mr. Guthrie, forms my communication. The interest which you have been good enough to take in my researches, which are so closely related to yours, will be my excuse for troubling you with them. If you think that these formulæ have an interest for the Physical Society, I beg you to communicate them in my name.

&c. &c.

PROFESSOR FOREL.

My dear Mr. Guthrie,

Morges, 4th Oct. 1876.

I kept back for some days the above letter (which I wrote on the 29th of September), because I had decided to verify some of the observations which did not agree with my formula. I have returned to day from Wesen, where I have been examining the Lake of Wallenstadt, and I am happy to inform you that the question which perplexed me is now fully cleared up.

According to my former observations, the duration of the seiches on the Lake of Wallenstadt was  $371''$ , the length being 15,000 metres. Calculations from the formula gave a mean depth of 128 metres. Now the only depth given, and which was considered the maximum, was 114 metres. Accordingly either the formula must be defective or the real depth must be greater. The latter is the case. Soundings which I took yesterday in this lake showed me that there is a great basin of comparatively even bottom, having at different points the depths of 97, 104, 118, 128, 133, 136, 138 metres. These, as you see, have a mean value which approaches much more closely to the value 120, which the formula gives.

I am happy in being able to give you this new confirmation of my hypothesis, and &c. &c.

H. FOREL.

LVI. *On the Application of Thermodynamics to the Study of the Variations of Potential Energy of Liquid Surfaces. Divers Consequences. (Preliminary Communication.)*  
By G. VAN DER MENSBRUGGE, Correspondent of the Royal Academy of Belgium\*.

SUPPOSE a liquid mass  $m$ , of which  $t$  is the absolute temperature,  $S$  the free surface, and  $T$  the potential energy per unit of surface; the total potential energy of the surface will be  $TS$ , exclusively of all other energy, such as that due to weight, to a change of volume, &c. Let us search out the quantity of heat  $dQ$  which the mass must supply for the surface  $S$  to receive an increment  $dS$ .  $dQ$  will evidently be a function of  $S$  and of  $t$ . Now, from the second principle of thermodynamics,  $\frac{dQ}{t}$  must be an exact differential  $d\mu$ ; let, then,

$$\frac{dQ}{t} = d\mu = XdS + YSdt,$$

$X$  being the variation of  $\mu$  when  $S$  is increased by the unit of surface, the temperature  $t$  remaining constant,  $Y$  the variation of  $\mu$  by unit of surface when  $S$  remains constant and  $t$  becomes  $t+1$ ; it is not difficult to see that  $X$  and  $Y$  are independent of  $S$ , and consequently we shall have

$$\frac{dX}{dt} = Y,$$

whence 
$$dQ = tXdS + t\frac{dX}{dt}Sdt.$$

If the increment  $dS$  is produced by the external work  $TdS$ , it is clear that  $dQ$  may be decomposed into two parts:—one,  $ATdS$ , corresponding to this external work; and the other,

$$tXdS + t\frac{dX}{dt}Sdt - ATdS,$$

which corresponds to the internal work  $dU$ , equivalent to  $AdU$ ,  $A$  being the thermal equivalent of the unit of work. We have therefore

$$AdU = (tX - AT)dS + t\frac{dX}{dt}Sdt,$$

together with the condition

$$\frac{d(tX - AT)}{dt} = t\frac{dX}{dt} \text{ or } X - A\frac{dT}{dt} = 0.$$

\* Translated from a separate impression, communicated by the Author, from the *Bulletins de l'Académie royale de Belgique*, 2<sup>m</sup>e série, tome xli. no. 4, April 1876.

From this we deduce

$$X = A \frac{dT}{dt}, \quad Y = A \frac{d^2T}{dt^2};$$

and consequently the variation of the heat  $Q$  contained in the mass  $m$  will be given by

$$dQ = AT \frac{dT}{dt} dS + At \frac{d^2T}{dt^2} S dt = At d \left( S \frac{dT}{dt} \right) \quad . \quad (1)$$

I have supposed the liquid surface in contact with air; but equation (1) evidently applies to the case of the surface of separation of two liquids which do not mix, or to that of the surface of contact of a solid and a liquid. I shall speedily treat the case in which  $S$  represents either the free surface of a solid body, the surface of separation of two solid bodies, the free surface of a gas, or the surface of contact of a solid and a gas. At present I shall deduce from this equation several consequences which appear to be of great import.

I. If we impart to a liquid mass  $m$  an increment  $dS$  of free surface, then the potential energy  $T$  is a positive quantity; that is to say, the total initial potential energy has received a positive increment  $TdS$ . It follows that the mass  $m$ , having acquired a greater potential energy, must have lost a certain quantity of heat; that is,  $dQ$  is negative. This is shown more simply still by the sign of

$$d \left( S \frac{dT}{dt} \right) \quad \text{or of} \quad \frac{dT}{dt};$$

the tension  $T$  diminishing when the temperature  $t$  increases,  $\frac{dT}{dt}$  and consequently also  $dQ$  must be negative. It was from this point of view that Sir W. Thomson investigated the thermal effect produced when a liquid film is stretched\*; but he has given only the term

$$At \frac{dT}{dt} dS$$

of the second member of equation (1).

I purpose to submit this formula to the test of numerous experiments; in this preliminary note I will merely say that equation (1) appears to me to account for a great number of phenomena yet unexplained. In order to judge of this by particular instances, let us replace  $dQ$  by  $mgkdt$ ,  $k$  being the specific heat of the mass  $m$  at the temperature  $t$ ; we shall

\* "On the Thermal Effect of drawing out a Film of Liquid" (Phil. Mag. 1859, [4] vol. xvii. p. 61). Compare the article entitled "Influence de la température sur les forces de réunion" (*Théorie mécanique de la chaleur*, par A. Dupré, p. 266: Paris, 1869).

then be able to write, for the variation of the temperature,

$$dt = \frac{At \frac{dT}{dt} dS}{mgk - At \frac{d^2T}{dt^2} S} \cdot \cdot \cdot \cdot \cdot \quad (1 \text{ bis})$$

This value shows that the less the mass  $m$  and the specific heat  $k$ , the more will the temperature vary.

Here are two curious facts, the explanation of which long puzzled me, and which, in my provisional opinion, proceed immediately from the above value of the variation of temperature  $dt$  :—

(1) In a bubble of soap-solution or of liquid glycerine, as is known, strange and altogether irregular motions are observed very energetic when the bubble has just been blown, and then less and less pronounced in proportion as the film gets thinner. Now the insufflation of the air (supposed to have the same temperature as the liquid) evidently produces a larger and larger free surface ; and consequently the mass must in its different points take temperatures as much lower as the fresh surface supplied by each portion of the film is more considerable. Owing to these differences of temperature, a multitude of currents will arise, now in one direction, now in the opposite. The continual descent of the liquid will maintain these phenomena for some time, until the extreme tenuity of the film renders relative displacements of the liquid more and more difficult and the motions less and less appreciable.

(2) When the liquid film produced by placing a drop of oil of turpentine upon the surface of distilled water has taken the white tint of the first order, and upon this film a fresh drop of turpentine is placed, this latter spreads out, like the first, in a coloured layer, and the surrounding film thickens as it is heaped up upon itself ; soon the layer is arrested for a few moments, and at length returns several millimetres towards its centre. Is not this the effect, on the one hand, of the spreading of the second drop, which cools its mass and raises its superficial tension, and, on the other, of the collecting upon itself of the surrounding film, a collecting which augments its temperature and diminishes its tension ? In this way a slight difference of tension may be annulled, or even change its sign ; and this would explain the fact which I observed long since without at that time surmising the cause.

I could cite many other facts which appear to me intimately connected with the cooling caused by variations of the free surface of a liquid mass ; but I prefer to study first these



phenomena carefully, reserving to myself to describe them in a more developed investigation.

II. If the increment of surface  $dS$  is produced by a solid body immersed in the mass  $m$  and not wetted by the liquid, a work  $T_1 dS$  must be developed,  $T_1$  designating the potential energy of the unit of surface of contact. Now I have demonstrated\* that this energy constitutes a real tension; it follows that in this case, again, the liquid mass will undergo a diminution of temperature which will be greater in proportion as the mass  $m$  and the specific heat  $k$  are less.

I shall shortly endeavour to prove this assertion by direct experiments; but, as I shall show further on, there are facts which demonstrate it indirectly.

III. Suppose in the third place, that the increment  $dS$  given to the surface is produced by a solid body plunged in the mass  $m$  and wetted by the liquid; then the potential energy  $T_2$  of the unit of surface of contact of the solid and liquid is not due to a force of tension, but to a force of *extension*, as I have sought to establish in the Note above cited. It hence follows that the potential energy of the mass, instead of being increased by the augmentation of the surface, is, on the contrary, diminished; and consequently this loss of potential energy must be compensated by a heating of the mass  $m$ . This follows immediately from formula (1), where  $\frac{dT}{dt}$  is then positive, and consequently also  $dQ$ .

This curious theoretical consequence seems fully verified by the numerous experiments made in 1822 by Pouillet†, who ascertained that heat is disengaged whenever a solid, previously reduced to powder or filings as fine as possible and then perfectly dried, is wetted with any liquid: he found, by delicate measurements, that glass becomes heated  $\frac{1}{4}^{\circ}$  Centigrade when wetted either with water, oil, alcohol, or acetic ether; under the same treatment porcelain receives a heating of about  $\frac{1}{2}^{\circ}$ ; lastly, clay becomes nearly  $1^{\circ}$  warmer when soaked with water, and  $\frac{3}{4}^{\circ}$  with acetic ether.

Formula (1) indicates that the more considerable the increment of the surface, of course up to a certain limit, the more energetic will be the disengagement of heat; now this, again, was verified by the experiments of Pouillet. Among the inorganic bodies which he tried, it was the most porous, such as clay (brick), which became the most heated; but

\* "Sur les propriétés de la surface de contact d'un solide et d'un liquide," *Bull. de l'Acad. Royale de Belgique*, tome xl. p. 341.

† "Mémoire sur de nouveaux phénomènes de production de chaleur," *Ann. de Chimie et de Physique*, 1822, tome xx. p. 141.

organic materials, vegetable or animal, gave much more marked results: thus paper, first dried and then impregnated with water, received a rise of temperature of  $4^{\circ}52$ ; sponge,  $1^{\circ}9$ ; starch,  $9^{\circ}7$ ; iris-root,  $6^{\circ}12$ ; liquorice-root,  $10^{\circ}2$ ; ox-tendon,  $3^{\circ}16$ ; and the very thin membranes of the intestines of sheep,  $9^{\circ}6$  with water, and  $10^{\circ}12$  with alcohol. These results, so surprising, compared with equation (1) appear to furnish, for the extreme tenuity of the ultimate organic particles into which liquids can penetrate, a proof at least unexpected, if not as certain as microscopic observations.

Pouillet's experiments were followed up in 1855 by M. Jungk\*, who established the following facts:—(1) in water absorbed by sand, there is depression or elevation of temperature, according as the initial temperature of the liquid is below or above  $+4^{\circ}$  C.; (2) the temperature is diminished in water at  $0^{\circ}$  absorbed by snow. These two facts, intimately connected with the abnormal expansion of water in the interval between its maximum of density and its freezing-point, appear to me again to spring immediately from equation (1). If my explanation were true, we should have the theory of regelation without the cooperation of external pressure. I reserve the examination of this point, again, for a special study.

But, in my opinion, the accuracy of equation (1) is shown especially by the excellent researches published in 1873 by M. Melsens†, which so well complete the preceding investigations. He has, in fact, found, as by an admirable intuition, the best conditions of the phenomena of imbibition: not only has he precisely estimated the quantities of material, solid or liquid, employed, as well as the intervals of time elapsed from the commencement of the absorption till the observation of each of the successive temperatures, but he has also operated on small quantities of solid material (charcoal) and slight quantities of various liquids. Thus, with 10 grammes of charcoal and 25 cubic centims. of liquid, the rise of temperature was  $4^{\circ}$  for alcohol,  $6^{\circ}$  for rectified ethylic ether, and  $17^{\circ}$  for sulphide of carbon; lastly, 11 grammes of charcoal and 97 grammes of liquid bromine supplied to M. Melsens a heating amounting to  $30^{\circ}$ , while only 4.45 grammes of charcoal and 33 grammes of bromine gave him, in a very short time, a rise

\* "Ueber Temperaturerniederung bei der Absorption des Wassers durch feste poröse Körper," *Pogg. Ann.* vol. cxxv. p. 292.

† "Notes chimiques et chimico-physiques, 5<sup>me</sup> Note, chap. 1: De l'élévation de température produite par l'imbibition du charbon par l'eau, l'alcool, l'éther éthylique, le sulfure de carbone et le brome," *Mém. de l'Acad. royale de Belgique*, collection in-8vo, t. xxiii.

of more than 35°. These last facts (which, I am sure, must have surprised the able experimenter) would be explained, according to equation (1 bis), by the threefold influence of the values of the liquid mass  $m$ , of its specific heat  $k$ , and of the wetted surface  $S$ .

IV. I now approach another class of phenomena, which is directly connected with equation (1). The nature of the thermoelectric currents developed by the action of heat, whether in one and the same conductor the parts of which do not all possess the same physical properties, or in heterogeneous conductors of which the surface of contact is heated, allowed me to presume that equation (1) could be transformed into another containing the difference of the electric potentials of two conductors on the two sides of their surface of contact, whether these conductors are heterogeneous or only differ in their physical properties. I was fortunate enough to see verified what I had conjectured, and thus to discover the physical cause of a numerous class of phenomena which have much perplexed those who discovered them.

To effect this transformation, I make use of two propositions borrowed from the theory of thermoelectric currents, and first demonstrated by M. Clausius :—

1. The difference of electric level  $x$  established at the contact of two heterogeneous bodies (or differing physically) is proportional to the absolute temperature of the contact.

2. The quantity of heat  $dQ$  which traverses in the time  $d\tau$  the surface of contact of the two bodies is equivalent to

$$Axi d\tau,$$

$A$  being the thermal equivalent of the unit of work,  $x$  the difference of electric level, and  $i$  the intensity of the current.

Having regard to these two propositions, I can therefore write, designating by  $\lambda$  a constant which depends on the nature of the two bodies :—

$$x = \lambda t, \quad dQ = A\lambda i t d\tau.$$

Introducing the value of  $dQ$  into equation (1), it becomes

$$A\lambda i t d\tau = A t \lambda \left\{ \frac{dT}{dx} dS + \frac{d^2T}{dx^2} S dx \right\},$$

or

$$i d\tau = d \left( S \frac{dT}{dx} \right) . . . . . (2)$$

I conclude from this transformed equation that to every variation of temperature of the surface of contact corresponds a variation in the difference of electric level, and consequently a thermoelectric current will be produced if the circuit be closed.

Before reporting the verification-experiments, I will mention that equation (2) is identical with that obtained by M. Lippmann\* for the quantity of electricity which passes through the surface of contact of mercury and acidulated water, of which surface  $T$  is the superficial tension (that is, the potential energy of the unit of surface),  $x$  the difference of electric level when the surface of contact  $S$  receives an increment  $dS$ .

The new interpretation which I have just given to equation (1), and which has permitted me to deduce very simply from it the formula (2) already obtained by M. Lippmann for a particular case, leads me to believe that the elegant experiments of that physicist, and those, of the same kind, published by M. Quincke†, are decidedly in favour of the theory of thermo-electric currents. If this assertion, rendered so probable by the preceding considerations, and supported besides by all the details of those experiments, should be confirmed by my subsequent researches, we shall perhaps recognize the true relations between thermal, electric, and capillary phenomena.

M. Lippmann has proved the existence of two very important laws, of which the following is the expression:—

1st. The capillary constant at the surface of separation of mercury and dilute sulphuric acid is a function of the electric difference which takes place at that surface.

2nd. When a liquid surface is deformed by mechanical means, the electrical difference of that surface varies in a direction such that the superficial tension developed opposes the continuance of the movement.

The preceding demonstration enables us to see that these two laws amount to the two following, at the same time clearer and more general; for they apply to all liquids, whether they have surfaces of contact with air, with other liquids, or with solids:—

1. The capillary constant at the free surface of any liquid, at the surface of separation of two liquids which do not mix, or at the surface of contact of a liquid with a solid, is a function of the absolute temperature of the surface. (This law, known long since, involves, pursuant to the first proposition of M. Clausius (see above), that of M. Lippmann, generalizing this latter.)

2. When, by mechanical means, a liquid surface is deformed, the temperature of this surface varies in a direction such that

\* *Relations entre les phénomènes électriques et capillaires*, 1873, Paris, chez Gauthier-Villars.

† "Ueber elektrische Ströme bei ungleichzeitigem Eintauchen zweier Quecksilber-Electroden in verschiedene Flüssigkeiten," *Pogg. Ann.* vol. cliii. p. 161.

the superficial tension developed opposes the continuation of the movement.

Let us now cite two verifying experiments described by M. Lippmann.

*Experiment I.*—Two glasses, both containing mercury covered with acidulated water, are placed side by side ; they are put into electrical communication by means of a cotton wick ; the two masses of mercury communicate with the exterior by means of platinum wires which do not touch the acidulated water. After putting these wires in communication with the extremities of a galvanometer-wire, the experimenter inclines one of the two glasses ; the needle of the galvanometer is immediately deflected, indicating a current passing through the liquid of the inclined glass towards the glass that remains upright.

By this operation the surface of contact of the mercury and acidulated water in the first glass is enlarged ; this surface must therefore be cooled, and the potential energy in it increased ; but, in return, the electrical difference there has diminished ; hence the current just indicated.

M. Lippmann adds that the electrification is in time dissipated ; this is quite natural, since the current arises only from a difference of temperature.

*Experiment II.*—Instead of inclining one of the glasses, the surface of the mercury contained in it can be enlarged by depressing it, dipping into it, for example, the end of a glass or wooden rod ; when this is done, we see the galvanometer-needle deflected immediately.

Thus my second consequence would be found fully confirmed relatively to the case of the augmentation of the surface of contact of two bodies which do not wet one another.

I cannot avoid recalling a very curious experiment made by M. Becquerel, because it furnishes a double verification of the theory. He puts one of the extremities of a very delicate galvanometer into communication with a platinum capsule, while the other communicates with platinum sponge freshly heated to redness ; he then fills the capsule with concentrated nitric acid, when he observes the following facts : “ the instant the immersion is effected the sponge receives from the liquid negative electricity, as if the platinum had been attacked ; it is immediately polarized so as to produce a current in the opposite direction, which continues for some time, diminishes, and becomes *nil* ” \*. Now does not the first current arise

\* *Traité de l'électricité*, tome ii. : see the chapter entitled, “ Effets électriques produits dans les actions capillaires.”

from a diminution of temperature due to the great increase of the liquid surface without the platinum sponge being wetted, thanks to the layer of air which covers it? But soon this air is expelled; and then a large surface is wetted by the liquid, whence results a rise of temperature, and consequently a current in the opposite direction and more energetic than the first.

V. Equations (1) and (2) appear to me to prove rigorously that every change in the surface of a liquid gives rise to a change of temperature, and, if the circuit is closed, to a thermoelectric current. From this point of view these equations must be of capital importance in meteorology. Indeed, if a mere drop of water falling into a pond, and there losing the whole of its original free surface, can produce, independently of the action of gravity, a diminution of potential energy, and consequently a heating, to which corresponds a variation in the electrical difference of the pond—if the smallest quantity of vapour that rises from a liquid produces a diminution of temperature and of electric difference, what powerful thermal and electrical effects have we not to expect from those immense variations of free surface in the waters that cover the earth and in the vapours that rise into the atmosphere? On the one hand, the waters of the sea are submitted, owing to the action of the sun, to continual evaporation, which causes the thermal and electrical state of the earth to be incessantly changing, and constantly develops in it thermoelectric currents; and on the other, the enormous quantities of vapour which rise into the atmosphere must there be subjected to incessant variations in their surface of contact with the air, from the state of extreme (I will say molecular) tenuity to that in which, through sudden condensation, they produce prodigious quantities of electricity and fall in torrents upon the surface of the globe from which they were raised. Thus, then, on the one hand would be found established the existence of a constant source of thermoelectric currents circulating in the earth; and on the other we should recognize a permanent cause of development of electricity in the atmospheric air, and the explanation of the enormous electrical discharges produced in tempests.

I deem that the preceding considerations collectively suffice to show the astonishing fecundity of the formula (1); only, as I desire to confirm more and more the consequences above indicated, and, if need be, to rectify them in certain points, I here conclude my preliminary communication, reserving for special memoirs the developments of the questions which I have raised.

LVII. *On some Blowpipe Reactions.* By E. J. CHAPMAN, Ph.D., Professor in University College, Toronto\*.

**O**N the Opalescence produced by Silicates in Phosphor-salt.—

It is well known that most silicates when fused with phosphor-salt are only partially attacked—the bases, as a rule, gradually dissolving in the flux, whilst the silica remains in the form of a flocculent mass technically known as a “silica skeleton.” Very commonly, almost invariably, indeed, if the blast be long continued, the bead becomes more or less milky or opalescent on cooling. This latter reaction was apparently regarded by Plattner as essentially due to the presence of alkaline or earthy bases, such as exhibit the reaction *per se*. He states (*Probirkunst*: Dritte Auflage, p. 468)—“Da man nun von mehreren Silikaten ein Glas bekommt, welches, so lange es heiss ist, zwar klar erscheint, aber unter der Abkühlung mehr oder weniger opalisirt, so muss man sich von der ausgeschiedenen Kieselsäure überzeugen, so lange das Glas noch heiss ist, und dabei die Loupe zu Hülfe nehmen. Die so eben erwähnte Erscheinung tritt gewöhnlich bei solchen Silikaten ein, deren Basen Kalkerde, Talkerde, Beryllerde oder Yttererde sind, die für sich mit Phosphorsalz, bei gewisser Sättigung des Glases, unter der Abkühlung oder durch Flattern milchweiss oder opalartig werden.” Dr. Theodor Richter, the editor of the 4th edition of Plattner’s work, leaves out the “gewöhnlich” of the above quotation, and so makes the implication still stronger. In this *vierte Auflage*, the statement runs—“Bei solchen Silikaten deren Basen für sich mit Phosphorsalz, bei gewisser Sättigung des Glases, unter der Abkühlung oder durch Flattern milchweiss oder opalartig werden (Kalkerde, Talkerde, Beryllerde, oder Yttererde) wird die Perle unter der Abkühlung mehr oder weniger trübe.” It is true enough that silicates in which these bases are present, exhibit the reaction; but as other silicates, practically all, indeed, exhibit the reaction also, the inference implied in the above statement is quite erroneous. The opalescence of the glass arises entirely from precipitated silica. If the blast be sufficiently kept up, a certain amount of silica is almost always dissolved; but this becomes precipitated as the glass cools. A simple experiment will show that this is the true cause of the opalescence. If some pure silica (or a silicate of any kind) in a powdered condition, be dissolved before the blowpipe-flame in borax until the glass be nearly saturated, and some phosphor-salt be then added, and

\* Communicated by the Author.

the blowing be continued for an instant, a precipitation of silica will immediately take place, the bead becoming milky—or, in the case of many silicates, opaque-white—on cooling. This test may be resorted to for the detection of silica in the case of silicates which dissolve with difficulty in phosphor-salt alone, or which do not give a well-pronounced “skeleton” with that reagent\*.

*On the Reactions of Chromium and Manganese with Carbonate of Soda.*—When a mineral substance is suspected to contain manganese, it is commonly tested by fusion with carbonate of soda. But chromium compounds form with that reagent a green enamel much resembling that formed by compounds of manganese.

The chromate-of-soda enamel, however, is yellowish-green after exposure to an oxidating flame; and the green colour never exhibits any tinge of blue.

The manganate-of-soda enamel, on the other hand, is generally greenish-blue when quite cold.

To avoid, however, any risk of error in the determination, the bead may be saturated with vitrified boracic acid until all the carbonic acid is expelled and a clear glass is obtained. The chrome glass will retain its green colour, whilst the manganese glass will become amethystine or violet. In place of boracic acid, silica may be used if more convenient. In this case, the reaction is assisted by the addition of a very small amount of borax.

*On the Detection of Cadmium in the Presence of Zinc in Blowpipe Experiments.*—When cadmiferous zinc-ores, or furnace-products derived from these, are treated in powder with carbonate of soda on charcoal, the characteristic red-brown deposit of cadmium oxide is generally formed at the commencement of the experiment. If the blowing be continued too long, however, this deposit may be altogether obscured by a thick coating of

\* By whom was the formation of a “silica skeleton” first made known? There is no reference to it in the early treatise of Von Engeström attached to his translation of Cronstedt's *Mineralogie* (edition 1, 1770; ed. 2, by John Hyacinth de Magellan, 1788), although phosphor-salt is mentioned as a reagent under the term of *sal fusibile microcosmicum*, and was indeed used by Cronstedt before 1758, the year in which his *Mineralogie* was anonymously published. Bergmann, who followed as a blowpipe worker, states that “siliceous earth” is very slowly attacked by microcosmic salt; but he does not seem to have remarked the skeleton-formation in the case of any silicate. The reaction appears to have been first definitely pointed out by Berzelius in his standard work on the blowpipe, published in 1821. It was therefore most probably discovered by him; or perhaps—as he lays no claim to its discovery, whilst claiming to be the originator of other tests—it may have been communicated to him by Gahn?



zinc oxide. When, therefore, the presence of cadmium is suspected in the assay-substance, it is advisable to employ the following process for its detection. The substance, if in the metallic state, must first be gently roasted on a support of porcelain or other non-reducing body. Some of the resulting powder is then fused with borax or phosphor-salt on a loop of platinum wire, and bisulphate of potash in several successive portions is added to the fused bead. The latter is then shaken off the wire into a small porcelain capsule, and treated with boiling water. A bead of alkaline sulphide is next prepared by fusing some bisulphate of potash on charcoal in a reducing flame, and removing the fused mass before it hardens. A portion of the solution in the capsule being tested with this, a yellow precipitate will be produced if cadmium be present. The precipitate can be collected by decantation or filtration, and tested with some carbonate of soda on charcoal. This latter operation is necessary, because if either antimony or arsenic were present, an orange or yellow precipitate would also be produced by the alkaline sulphide. By treatment with carbonate of soda on charcoal, however, the true nature of the precipitate would be at once made known.

*On the Solubility of Bismuth Oxide in Carbonate of Soda before the Blowpipe.*—Neither in the treatise of Berzelius, nor in the more modern and advanced work of Plattner, is any reference made to the behaviour of oxide of bismuth with carbonate of soda in an oxidating flame. In Plattner's *Tabellarische Uebersicht des Verhaltens der Alkalien, Erden, und Metalloxyde für sich und mit Reagentien im Löthrohrfeuer*, whilst oxide of lead is stated, correctly, to be soluble in carbonate of soda in an oxidating flame, the reference to oxide of bismuth is simply, that with carbonate of soda on charcoal it becomes immediately reduced to metallic bismuth; and none of his translators seem to have thought it necessary to supply the omission. In Hartmann's tabular *Untersuchungen mit dem Löthrohr*, in the handy little work of Bruno Kerl (*Leitfaden bei qualitativen und quantitativen Löthrohr-Untersuchungen*), in the *Löthrohr-Tabellen* of Hirschwald, and all other blowpipe books that I have met with, the same singular omission occurs. This seems to bear out very forcibly the somewhat cynical adage that "books are made from books." To supply the omission, it may be observed that bismuth oxide dissolves in carbonate of soda very readily in an oxidating flame, if the supporting agent be platinum wire or other non-reducing body. The glass is clear yellow whilst hot, but on cooling it assumes an orange or yellowish-brown colour, and becomes pale-yellow and opaque when cold. As regards their solubility by fusion

in carbonate of soda, metallic oxides fall into three groups :— (1) *Easily soluble*, e. g.  $\text{PbO}$ ,  $\text{Bi}_2\text{O}_3$ ,  $\text{BaO}$ , &c.; (2) *Slightly or partially soluble*, e. g.  $\text{Mn}_2\text{O}_3$ ,  $\text{CoO}$ , &c.; and (3) *Insoluble*, e. g.  $\text{Fe}_2\text{O}_3$ ,  $\text{Ce}_2\text{O}_3$ ,  $\text{NiO}$ ,  $\text{CaO}$ ,  $\text{MgO}$ , &c.

*On the Detection of Bromine in Blowpipe Experiments.*—When fused with phosphor-salt and copper oxide, the bromides, it is well known, impart an azure-blue coloration to the flame, much like that produced by chlorides under similar treatment, although streaked more or less with green, especially at the commencement of the operation. To distinguish these bodies more closely, Berzelius recommended the fusion of the test substance with 6 or 7 volumes of bisulphate of potash in a closed tube. Bromides by this treatment become decomposed, as a rule, and give off strongly smelling brownish or yellowish-red vapours of bromine. But this process does not always give satisfactory results, as in some instances the bromide is very slightly attacked. In this case, the following method, based on a peculiar reaction of bromide of silver, first pointed out by Plattner, may be resorted to. If insoluble, the bromide is fused with 2 or 3 volumes of carbonate of soda. A soluble bromide of sodium is thus formed, with separation of the base. To the filtered or decanted solution of the fused mass a small fragment of nitrate of silver is added, in order to precipitate bromide of silver. This, collected by decantation, is fused with a small quantity of bisulphate of potash in a little flask or test-tube. The bromide of silver will quickly separate from the flux in the form of a blood-red globule, which becomes pale-yellow when cold. The little globule, washed out of the tube by dissolving the fused bisulphate in some warm water, is carefully dried by being rubbed in a piece of blotting or filtering paper, and is then placed in the sunlight. After a short time it will turn green. Chloride of silver, as obtained in a similar manner, melts into an orange-red globule, which changes to clear-yellow on cooling, and finally becomes white, or nearly so. Placed in sunlight it rapidly assumes a dark-grey colour. Iodide of silver, under similar treatment, forms whilst hot an almost black globule, which becomes amethyst-red during cooling, and dingy-yellow when cold. In the sunlight it retains the latter colour. A mixture of chloride and iodide of silver assumes a greenish tint somewhat resembling the colour acquired by the bromide globule. This, however, can scarcely give rise to any error, as the presence of iodine is revealed, even if no violet-coloured fumes be emitted, by the dark amethystine colour of the bead whilst hot.

*On the Detection of Carbonates in Blowpipe Practice.*—A

mineral substance of non-metallic aspect, in nine cases out of ten, will be either a silicate, sulphate, phosphate, borate, carbonate, fluoride, or chloride—more especially if the streak be uncoloured or merely exhibit some shade of green or blue, or if the substance evolve no fumes when heated on charcoal.

Simple fusion with phosphor-salt on a loop of platinum wire serves at once to distinguish a silicate from any of the other bodies enumerated above, as whilst the silicate is but slowly attacked, these other bodies are readily and rapidly dissolved. Among the latter, again, the carbonates are distinguished without risk of error by the marked effervescence which they produce in the bead by the evolution of carbonic acid during fusion, the phosphates, sulphates, &c. dissolving quietly. The reaction is quite as distinctive as that produced by the application of an ordinary acid; but, of course, it may arise in both cases not only from a carbonate proper, but from the presence of intermixed calcite or other carbonate in a silicate or other body. It was by its use, upwards of twenty years ago, that the writer detected the presence of carbonate of lime in certain specimens of Wernerite (the “Wilsonite” variety), portions of which had been previously analyzed without the impurity having been discovered. It need scarcely be stated that the test-substance must be added to the phosphor-salt, on the platinum loop, only after the quiet fusion of the flux into a transparent glass. The reaction is, of course, manifested equally well with borax\*.

*On the Uselessness of Turner's Flux as applied to the Detection of Boracic Acid.*—Many years ago (about 1827 or 1828) Turner proposed, in examining a body for the presence of boracic acid, to mix the test-substance with bisulphate of potash and fluor-spar (in the proportions of  $4\frac{1}{2}$  parts of the former to 1 part of the latter), and to expose the mixture on a clean platinum wire to the point of the blowpipe flame. Fluoboric acid is thus produced; and by its volatilization a momentary green colour is imparted to the edge of the flame. Merlet recommends the employment of 3 or 4 parts of this flux to 1 part of the substance under examination. This test is much quoted in blowpipe books and works on chemical analysis generally; but it is altogether superfluous. With borate of soda it fails entirely, or yields very unsatisfactory results; and although it answers for most other borates and for borosilicates, it is uselessly applied to them, because these bodies colour the flame

\* It is singular that this very marked and useful reaction should not have been alluded to in any of the standard treatises on blowpipe practice. The only work known to the writer in which a passing reference is made to it, is that of Hirschwald (*Löthrohr-Tabellen*), published in 1875. The present writer called attention to it in 1871.

equally well *per se*. Berzelius seems strangely to have overlooked the coloration of the flame as produced by many substances under blowpipe treatment. In his work on the blowpipe, for example, he fails to notice the character in describing the reactions of lepidolite, sulphate of baryta, datolite, triphylline, and other minerals, which exhibit it most distinctly. Under axinite, moreover, he has the following statement:—"Turner asserts that a flame tinged green by boracic acid is obtained by the aid of sulphate of ammonia (or bisulphate of potash) and fluor spar." This "assertion" is true enough; but *all specimens of axinite colour the flame green per se*. The uselessness of the flux was pointed out, I find, by Buzengeiger as long ago as 1829. In the *Annales des Mines* for that year (tome v. p. 36), he states:—"J'ai essayé, pour reconnaître la présence de l'acide borique, d'employer le flux indiqué par M. Turner, mais ces tentatives ne m'ont pas réussi, probablement par défaut d'habitude. Quoi qu'il en soit, tous les minéraux que M. Turner a vu colorer la flamme en vert en les mêlant avec son flux, m'ont donné la même réaction en les introduisant avec quelque soin dans la flamme bleue, sans les mélanger avec aucun réactif." Buzengeiger, whose name does not seem to be quoted in any blowpipe work, appears to have first proposed the sloping the blowpipe-wick long before it was adopted by Plattner; and he noticed, at the same early date, that the crimson coloration of the strontium-flame was entirely obliterated by the presence of barytic compounds.

*On the Comportment of certain Alloys under the Action of the Blowpipe.*—In examining these reactions, about equal portions of the metals (forming the alloy) may be placed together on charcoal, and subjected to the action of a reducing flame.

1. Platinum and Tin unite with violent deflagration and emission of light, forming a hard, brittle, and infusible globule.

2. Platinum, Zinc and Tin unite with violent action, the zinc throwing off long flakes of oxide.

3. Platinum and Zinc, *per se*, do not combine, the zinc burning into oxide.

4. Platinum and Lead unite quietly, forming a brittle globule.

5. Platinum and Thallium unite quietly; the resulting globule is dark externally, grey internally, and quite brittle.

6. Platinum and Bismuth unite quietly, or with merely slight spitting, into a dark brittle globule.

7. Platinum and Copper combine quietly, though not very readily, into a hard, light-coloured, malleable globule.

8. Platinum and Silver unite quietly, but not very readily unless the silver be greatly in excess, into a white malleable globule.

9. Platinum and Gold unite quietly, forming (if the gold be somewhat in excess) a yellow malleable globule.

10. Gold and Tin unite quietly into a very brittle globule.

11. Gold and Zinc do not combine *per se*; the zinc burns into oxide.

12. Gold and Lead combine quietly, forming a grey brittle bead.

13. Gold and Thallium unite quietly, but separate again to some extent during cooling. The globule may thus frequently be flattened out, but not without cracking at the sides. If the metals remain united, the button is dark blackish grey and quite brittle.

14. Gold and Bismuth unite quietly and readily, forming a very brittle globule.

15. Gold and Copper, and 16. Gold and Silver, unite and form a malleable globule.

17. Silver and Tin unite quietly into a malleable globule.

18. Silver and Lead unite readily into a malleable globule.

19. Silver and Thallium combine readily; globule malleable.

20. Silver and Bismuth unite readily and quietly: the globule is brittle, but admits of being slightly flattened out.

21. Silver and Copper, and 22. Silver and Gold, form malleable globules. The gold alloy, even with gold largely in excess, is quite white. If it be flattened out and heated in a platinum spoon with some bisulphate of potash, it will become yellow from the silver on the surface being dissolved. On remelting the flattened disk, a silver-white globule is again obtained.

23. Copper and Tin unite into a grey and partially malleable bead, the surface of which, in the oxidizing flame, becomes more or less thickly incrustated with cauliflower-like excrescences of oxide.

24. Copper and Zinc do not unite *per se* into a globule, the zinc burning into oxide. Under carbonate of soda, or carbonate of soda and borax, brass is readily formed.

25. Copper and Lead form a dark-grey globule, which is sufficiently malleable to admit of being extended on the anvil.

26. Copper and Thallium melt into a dark-grey malleable globule.

27. Lead and Tin unite readily; but the globule commences

immediately to oxidize, throwing out excrescences of white and yellow oxide. On removal from the flame it still continues in ignition, and pushes out further excrescences. The unoxidized internal portion (if any remain) is malleable.

28. Lead and Bismuth unite readily: the molten globule acquires a thin dark coating of oxide on the surface only, and admits of being flattened out, more or less, upon the anvil.

29. Lead and Thallium form a malleable globule.

30. Bismuth and Tin unite readily; but the fused mass immediately throws out excrescences, and becomes covered with a dense crust of oxides. The reaction, however, is not so striking as with lead and tin.

31. Thallium and Tin exhibit the same reaction as lead and tin; but the cauliflower-like excrescences are brownish black.

### LVIII. *Proceedings of Learned Societies.*

#### ROYAL SOCIETY.

[Continued from p. 394.]

April 6, 1876.—Dr. J. Dalton Hooker, C.B., President, in the Chair.

THE following paper was read:—

“Experiments on the Friction between Water and Air.” By Dr. Ritter von Lang.

The method adopted for estimating the mutual friction of water and air consisted in connecting a glass tube of 8 centims. length and 0.72 internal diameter with the pipes which supply Vienna with water at a pressure of four atmospheres. Arrangements for securing a vertical position for the tube ensure a perfectly continuous jet, devoid of any broken surface; and a glass tube surrounding this jet, with its axis coinciding with that of the jet, acts as an aspirator, into and along which air is drawn through a lateral feeding-tube. The amount of this indrawn air corresponding to the fall of a given amount of water was determined by observing the rate at which a film of soap was borne along the feeding-tube; and the velocity of the water causing the indraught was calculated from the diameter of the water column and the quantity of water discharged along it in a given time; but after having once determined the form of the slightly conical water column, the amount of water discharged was the only datum required for the calculation.

The influence of a greater or less section of the air-feeding-tube on the volume of the aspirated air was carefully determined, while also the absence of any appreciable retardation due to the soap film was established.

Neglecting the slightly conical character of the surface of the water column, and assuming (as the result of experiments in which the motion of a smoke cloud was observed) that the movement of the air was throughout in lines parallel to the axis of the tube along which it flowed, and showing that the pressure does not vary along the length of the tube, the author proceeds to discuss the hydrodynamic equations expressing the conditions of the problem (the motion of the air being uniform and independent of time), and represents the volume of air  $A$  passing through the tube in a second as :—

$$A = W \left[ \frac{R^2 - r^2}{2r^2(\log R - \log r)} - 1 \right],$$

$W$  being the weight of water, in grammes, discharged in a second,  $r$  the radius of the jet in turns of the micrometer-screw (6·8 turns of which correspond to 1 centim.),  $R$  being the radius of the aspirating tube.

The results obtained by observation accorded well with those given by this equation, so long as the value of  $R$  did not exceed the limit within which the suppositions regarding the motion of the air hold good.

The question was considered whether the results might not be brought into even closer accord with theory by the assumption that a slipping action takes place between the air and the water-jet on the one hand, and between the air and the tube on the other, instead of the assumption previously made that the air adhered alike to the water and to the tube in its passage. The result of the calculation, however, led to no nearer approximation; and, finally, experiments with other materials for the tube and other gases (namely, coal-gas and carbonic anhydride) were made, without resulting in any marked difference from the results obtained with air and glass.

May 4.—Capt. F. J. O. Evans, R.N., C.B., Vice-President, in the Chair.

The following paper was read :—

“On the Absorption-Spectra of Bromine and Iodine Monochloride.”  
By H. E. Roscoe, F.R.S., and T. E. Thorpe.

The paper contains the results of an exact series of measurements of the absorption-spectra of the vapours of the element bromine and of the compound iodine monochloride, made with the object of ascertaining whether the molecules of these two gases vibrate identically or similarly, their molecular weights and colour of the vapours being almost identical. The two spectra, which are both channelled, were compared simultaneously by means of one of Kirchhoff's 4-prism spectroscopes, the position of the lines being read off by reflection on an arbitrary scale. In order to determine the wave-lengths of these bands, the wave-length of each of 27 air-lines lying between the extremes of the absorption-spectra was ascertained by reference to Thalén's numbers; whilst for the pur-

pose of reducing the readings of the absorption-bands to wave-lengths a graphical method was employed, the details of which are given in the paper. This method appears to be one of general applicability for the plotting of spectra.

Tables then follow giving the wave-lengths of 66 bands of each absorption-spectrum; and a map accompanies the text, in which the bands are drawn to a scale one half that of Ångström's "Spectre Normal."

A careful comparison of these Tables and of the map shows that, although both spectra contain a large number of lines which are nearly coincident, the spectra as a whole are not identical, either when the vapours are examined at high or low temperatures, or when the lengths of the columns of absorbing gas are varied.

#### GEOLOGICAL SOCIETY.

[Continued from p. 397.]

June 21, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

5. "On the Discovery of Plants in the Lower Old Red Sandstone of the Neighbourhood of Callander." By R. L. Jack, Esq., F.G.S., and R. Etheridge, Jun., Esq., F.G.S.

6. "On an adherent Form of *Productus* and a small *Spiriferina* from the Lower Carboniferous Limestone Group of the East of Scotland." By R. Etheridge, Jun., Esq., F.G.S.

7. Notice of the occurrence of remains of a British fossil *Zeuglodon* (*Z. Wanklyni*, Seeley) in the Barton Clay of the Hampshire coast." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

8. "On the remains of *Emys hordwellensis*, from the Lower Hordwell beds in the Hordwell Cliff." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

9. "On an associated Series of cervical and dorsal Vertebrae of *Polyptychodon* from the Cambridge Upper Greensand." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

10. "On *Crocodylus icenicus* (Seeley), a second and larger species of Crocodile from the Cambridge Upper Greensand." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

11. "On *Macrurosaurus semnus* (Seeley), a long-tailed animal with procœlous vertebrae, from the Cambridge Upper Greensand." By Harry Govier Seeley, Esq., F.L.S., F.G.S.



13. "On the Metamorphic Rocks surrounding the Land's-End mass of Granite." By S. Allport, Esq., F.G.S.

In this paper the author described the results of a microscopic examination of certain metamorphic rocks surrounding the Land's-End granite, indicating the changes produced by the intrusion of the latter upon clay slate and upon certain igneous rocks. The slates in contact with granite become converted into tourmaline- and mica-schists, and are found to contain crystalline quartz, tourmaline, and three distinct varieties of mica, with occasionally tremolite, magnetite (and andalusite?), and in some localities feldspar. Their structure is also changed, the most remarkable changes being foliation, with every gradation from nearly straight parallel lines to the most complicated contortions—and concretionary structure by segregation of quartz and mica, the result being a spotted schist. The strata near the granite contain far more quartz than those at a distance; and the author thought that there could be no doubt that much of the quartz has been derived directly from the intruded rock. He referred particularly to the fluid-cavities contained in the quartz of the granite, schorl-rock, and altered slates, and, from his observations upon them, stated that he was compelled to dissent from the views of Mr. Sorby, inasmuch as he found no uniformity to prevail in the relative sizes of the bubbles and fluid-cavities in the quartz crystals belonging to precisely the same portion of rock, or even in the same crystals. Hence he regarded it as impossible to arrive at even an approximate estimate of either the temperature or the pressure under which a given rock was formed from a consideration of such characters. The author next described the characters and mode of occurrence of tourmaline both in the granite and in the schorl-rock, and inferred that at the separation of the latter from the former the whole mass was in a plastic state, and that then the tourmaline and quartz became crystallized in an order varying in accordance with varying conditions. He also noticed the alteration of tourmaline producing pseudomorphs of that mineral; and stated that while all the three varieties of mica found distinctly furnish the red lithium-line when treated spectroscopically, this is most strongly marked and persistent in the white variety, which is probably a typical lepidolite.

The altered dolerites and basalts described by the author in the remainder of his paper are marked as "Greenstones" on the map of the Geological Survey. They were stated to vary in colour from dark bluish green to dark brownish green, and in texture from coarsely crystalline rocks, not fissile in any direction, to fine-grained or compact rocks with an imperfect slaty cleavage. The coarsely crystalline rocks are regarded by the author as altered dolerites, and some, if not all, of the more compact varieties as originating from fine-grained basaltic portions of the same rocks. The author was unable to decide whether these masses are contemporaneous and interbedded, or intrusive sheets (both may be present); but in any case they are older than the granite. He described in some detail the structure of specimens of these altered rocks from many localities,

the microscopic examination of which shows that the pyroxenic mineral, whether augite or diallage, has frequently been converted into a hornblendic substance, and that actinolite is found filling cavities and fissures in precisely the same manner as other products of alteration. The imperfect cleavage of the more compact varieties is regarded by the author as in accordance with the facts observed in typical slates. The metamorphism of the dolerites is regarded by him as different in kind from that of the slates, and caused rather by a decomposition and rearrangement of mineral substances *in situ* than by an introduction of new material; in many cases the process of alteration may be followed step by step; and from the evidence it would appear that two rocks of similar origin and composition may follow two different lines of metamorphosis, and thus become converted into two totally different substances; and, again, many of the metamorphic rocks have undergone a second series of changes, brought about chiefly by chemical forces, and indicated by the occurrence of micaceous and chloritic pseudomorphs after tourmaline and an alteration (hydration) of the mica.

With regard to the origin of the granite of Cornwall, the author said that neither observation in the field nor microscopical study lends any support to the notion that it is a metamorphic rock—but on the contrary, that there is the clearest evidence of former deep-seated volcanic action in the disturbance and alteration described in his paper, and in the enormous number of granitic and felsitic dykes intersecting the country for miles. The mode of occurrence of granite in other localities also seems to him to furnish evidence in the same direction.

14. “On the relation of the Upper Carboniferous strata of Shropshire and Denbighshire to beds usually described as Permian.” By D. C. Davies, Esq., F.G.S.

The author described sections obtained at the Ifton Rhyn Collieries and other places in the neighbourhood, and compared these with other sections displayed in different parts of England and the continent, and also in Nova Scotia, and stated his conviction that from the *Spirorbis*-limestone upwards, to and including the Permian, we have one continuous series of deposits. He maintained that there is no real general break in the sequence of the strata or in the continuity of life, but only local unconformabilities marked by equally local gaps in the succession of life.

15. “Notes on the Physical Geography and Geology of North Gippsland, Victoria.” By A. W. Howitt, Esq., F.G.S.

The earliest formation of which any trace is left in this district is the Silurian—all traces of any older rocks being removed, probably by the same agencies which have contorted and metamorphosed the Silurian slates and sandstones. The surface of all these Silurian strata shows signs of great denudation previous to the deposition of the Devonian. The period that elapsed between these two epochs

was one of volcanic activity, apparently subaerial and terrestrial, and representing the Lower Devonian. The Middle Devonian strata consist of shales and sandstones devoid of any traces of volcanic action, which, however, again becomes apparent in the Upper Devonian. The latter consists of conglomerates, sandstones, and shales, interstratified with aqueous deposits. The prevailing red colour of these beds the author suggests may possibly indicate lacustrine rather than marine conditions.

The next in the series of deposits present in North Gippssland are of Tertiary age, and rest horizontally on the flanks of the mountains at elevations nowhere exceeding 1000 feet. At the close of the Miocene and at the commencement of the Pliocene period the land probably was from 300 feet to 400 feet lower than at present.

The fact that different genera of fish are found in the streams flowing from the north and south sides of the Australian Alps indicate the high antiquity of that watershed. These mountains have been formed by the gradual elevation of the land *en masse*, and its equally gradual erosion by the streams and rivers.

16. "Further Notes on the Diamond-Fields &c. of South Africa."  
By E. J. Dunn, Esq.

These notes are intended to serve as additions and corrections to the author's paper read in 1873.

Further mining operations at De Beer's show that the "pipes" are more recent than the sheet of dolerite and other intrusive rocks surrounding them. At this mine the only instance of a dyke traversing the "pipe" occurs. Though the rock composing it was too much decomposed to be determined, it was still quite distinct. As the mines are worked out they disclose a form less circular than at first supposed.

The first effect of the disruptive force was to cause a rent in the rocks, the sides of which were afterwards torn away in the weaker parts by the intrusive rock until the present form resulted.

The "cores or pipes" have decomposed downwards, being darker-coloured and less decomposed at 80 or 100 feet from the surface and towards the centres. Several small freshwater shells were discovered in Kimberley Mine, in an apparently undisturbed deposit.

Gold is mined for at Leydenburg and Eersteling, in the Transvaal Republic. The rocks in which it occurs are generally barren-looking, and vary very much in strike. The alluvial gold at Leydenburg has doubtless been supplied from two distinct sources; it is coarse and nuggetty as a rule, well rounded, and generally coated with oxide of iron. Lumps up to 10 lbs. weight have been found; it is of good quality, worth from 76 fr. to 80 fr. per oz. The auriferous rocks at Eersteling are steatitic and chloritic schist resting on gneiss, and overlain by rocks which at Leydenburg are auriferous.

Cobalt is found near Oliphant River, in fine-grained felsite rocks. It does not occur in lodes, but in small threads and lenticular veins, running parallel to a dyke of fine-grained dolerite. The widest

vein of ore was 8 inches thick. More than 100 tons of ore have been sent to London.

17. "On Chesil Beach, Dorsetshire, and Cahore Shingle Beach, Co. Wexford." By G. H. Kinahan, Esq., M.R.I.A., &c.

The author carefully compares the situations, structures, &c. of these two shingle beaches, and points out that their wonderful similarity is due to nearly the same natural causes in each case, but that at Chesil the driftage is due to the flow-tide current augmented by waves caused by the prevailing winds, while at Cahore the driftage is solely due to the flow-tide currents, its effects being modified by adverse wind-waves. The sorting of the pebbles on Chesil Beach is probably chiefly caused by the progressive increase in the velocity of the tidal current as it approaches the nodal or hinge-line of the tide in the English Channel. The author considers that the current due to the flow of the tide has greater drifting-power than wind-waves.

18. "Some recent Sections near Nottingham." By the Rev. A. Irving, B.A., B.Sc., F.G.S.

The author describes a section of the strata exposed during the recent construction of a railway line from Carlton, three miles to the east of Nottingham, through Daybrook, to Kimberley.

The section commences in the Upper Keuper; the Lower Keuper (Waterstones) are penetrated at a short distance from the village of Gedling. These beds are well shown as the cutting enters the plateau known as the "Mapperley Plains," and consist of a series of alternating red marls and sandstones. One of the sandstone-beds is nearly white; and hollows varying in size from that of a pea to that of a fowl's egg are of frequent occurrence. These are probably due to the dissolving out of small calcareous concretions. This bed occurs high up in the Waterstone series. The total thickness of the Lower Keuper may be estimated at about 100 feet. At about one quarter of the height from the bottom ripple-marks are abundant; sun-cracks, rain-pittings, and pseudomorphs also occur; but as yet no footprints have been found.

The line then crosses about 3 miles of Bunter country. In the upper portion of the series the pebble-beds appear to be wanting, and their place is taken by a series of thinly laminated, micaceous, whitish, false-bedded sandstones. These are succeeded by marine conglomeratic beds, which extend for some distance. A mile further west the sandstones become softer, and assume a more mottled appearance, which they retain all the way to the Leen valley, where the base of the Bunter is hidden by alluvium.

The cutting then passes through an outlier of Lower Bunter and Upper "Permian" Sandstone. Two faults occur at this spot, by the first of which the Permian is thrown down several yards, whilst by the second it is let down below the level of the railway.

Near the village of Watnall a tunnel pierces the Magnesian Limestone and the Coal-measures. This junction is distinctly uncon-

formable, the Permian formation being nearly horizontal, and the Coal-measures dipping at an angle of  $15^{\circ}$  to the north-east. At the western end of the tunnel another fault lets down the Permian 12 feet; the Coal-measures are consequently lost to sight, until again brought up by another fault on the other side of the village. The author considers that the Permian strata are closely related to the overlying Trias, and are unconformable to the Coal-measures.

Here and there patches of drift were met with, but almost entirely in the Bunter country, owing to the friable nature of that deposit. These patches are probably the remnants of a drift once more widely spread.

Traces of drift on the Keuper country are remarkably few. Two patches have been preserved by a fault on the north-eastern side of Nottingham.

In a cutting on a new line of the Midland Railway it is found that the *Avicula-contorta* shales have been removed by evident glacial action, and redeposited among other materials on the planed and striated surface of the indurated greenish-grey marls of the lowest Rhætic strata.

19. "On the Permians of the North-east of England and their Relations to the under- and overlying Formations." By E. Wilson, Esq., F.G.S.

The author describes, as follows, the same section as that noticed in the preceding paper:—Lower Bunter, 30 feet; breccia of various slates and grits &c., underlain by red and yellow mottled and grey sandstones, considered by the author to be passage-beds between the Bunter and the Permian formations; these are followed by ordinary Permian marls, 21 feet; Magnesian Limestone, 33 feet; slate-coloured thin-bedded sandstones and shales, with a breccia at the base, 19 to 20 feet. The breccia rests in a series of very gentle undulations on a planed-off surface of Coal-measure shales which dip in a north-easterly direction at an angle of  $15^{\circ}$ . This unconformity between the Coal-measures and the almost horizontal Permians is most pronounced, a fact long ago pointed out by Prof. Sedgwick, and tends to confirm the supposition of the pre-Permian elevation and denudation of the Penine axis.

The author considers the breccia at the base of the Permian, and possibly that of the Lower Bunter, to be of glacial origin. The paucity of life in the intervening beds might thus be accounted for.

20. "The Section at High Force, Teesdale." By C. T. Clough, Esq., B.A., F.G.S.

The section described by the author (in descending order) is as follows:—Basalt, 24 feet; altered shale,  $1\frac{1}{4}$  foot; basalt, 6 feet; altered shale, 12 feet; limestone, 32 feet. The second stratum of basalt has been erroneously described in Phillips's 'Geology of Yorkshire' as "plate" or shale, whereas it is merely an underflow of the basalt. About 20 yards below the Force the lower and intercalated

layer of basalt has entirely disappeared. These intercalated (not interstratified) beds of volcanic rock are common throughout the district, and tend to prove the irruptive nature of the Whin Sill, as maintained by the late Prof. Sedgwick.

21. "The Distribution of Flint in the Chalk of Yorkshire." By J. R. Mortimer, Esq.

The author considers that the present shape of the Chalk Wolds of Yorkshire seems to suggest that they are the remains of an atoll or circular reef, probably one of a chain, rather than the fragment of a vast sheet of Cretaceous mud deposited in deep water. He thinks that the flint-bearing and non-flint-bearing chalk areas are in the main contemporaneous in Yorkshire. The chalk without flint contains 4.28 per cent. of silica, whilst the chalk with flint contains only 2.12 per cent.

22. "On the Mode of Occurrence and Derivation of Beds of Drifted Coal near Corwen, North Wales." By D. Mackintosh, Esq., F.G.S.

Resting on the lowest drift, which here, at an elevation of 500–600 feet above the level of the sea, consists of yellowish clay alternating with beds of coarse gravel, is a deposit of clean sand and fine gravel, containing streaks and layers of coal, which varies in form from fine dust to large lumps. The fine gravel appears to be entirely made up of local micaceous Silurian grit.

The glacial striæ run in a W.S.W. direction, and at first led the author to infer that the coal had been drifted from that quarter. This, however, would have involved the supposition that a portion of the Coal-formation *in situ* had been faulted down on the top of the remarkable outlier of Mountain Limestone which occurs about a mile and a half west of Corwen. The author is therefore now of the opinion that the coal was drifted from the Ruabon district during a comparatively temperate interglacial period. The land not being submerged beyond a few hundred feet, it was floated along the sinuosities of the valley of the Dee, and stranded in the shallow water of the Corwen area. Hence the transportation of débris may at times have taken place in a direction diametrically opposite to that of the glaciation of a district.

23. "The Cephalopoda-beds of Gloucester, Dorset, and Somerset." By J. Buckman, Esq., F.G.S.

The author considers the Cephalopoda-beds of Bradford and Dundry to be on the same horizon, and that neither the one nor the other have the slightest connexion with the Cephalopoda-bed of Gloucestershire. The Dorsetshire Cephalopoda-bed is the equivalent of the "Gryphite Grit" at Leckhampton; and the overlying roughly bedded stone is the representative of the "Trigonia Grit" of Cold Comfort. The Gloucestershire Cephalopoda-bed lies at the base of the Inferior Oolite, whilst the Dorsetshire bed is at the top of that formation.

Part of the sands at Bradford Abbas really represent the lower-oolitic mass of Leckhampton and Crickley; in fact the Dorset sands represent the lower freestones of Gloucestershire. The beds underlying the Dorsetshire Cephalopoda-bed are the equivalents of the Inferior Oolite of Ham Hill.

The author gives lists of the fossils from the freestone of Ham Hill, and of the Cephalopoda from the Dorset equivalent of the "Gryphite Grit." The latter do not appear in zones; but different species are prevalent at different localities, all on the same horizon. The other fossils from this bed point to the same conclusions, as the Ammonites are numerous and in good preservation.

24. "Evidence of the Subsidence of the Island of Guernsey." By R. A. Peacock, Esq., C.E., F.G.S.

All round the coast of this island, like that of Jersey, are found tree-trunks and other vestiges of old forest-land now submerged. Passages are quoted by the author from various old historians relative to the former existence of this tract as dry land, the submergence of which probably took place in the fifteenth century. The encroachment of the waters is due to the subsidence of the land, and not, as has been suggested, to the breaking in of the sea through some natural barrier upon some already low-lying district. Judging from the old chart of 1406, the amount of depression is equal to 160 feet.

## LIX. *Intelligence and Miscellaneous Articles.*

### ON THE ROTATORY POLARIZATION OF QUARTZ.

BY J. L. SORET AND ED. SARASIN.

IN a former memoir on the rotatory polarization of quartz\*, we announced the intention of extending our observations to the ultra-violet rays more refrangible than the line N, and of giving at the same time to our measurements a higher degree of precision. It is the results of these fresh researches that we have now the honour of communicating to the Academy.

To determine the angle through which quartz rotates the plane of polarization of rays of various wave-lengths we have, as before, operated upon solar light by the method of MM. Fizeau and Foucault. The left-handed quartz crystal which we have chiefly made use of has been retouched in its cutting so as to render its faces perfectly parallel, and perpendicular to the crystallographic axis. Its thickness is 29.885 millims. We have carefully determined its temperature for each measurement, and corrected the obtained values of the angle of rotation by reducing them, with the aid of the formula given by Von Lang, to the temperature of 20°.

\* *Comptes Rendus de l'Académie des Sciences*, Oct. 11, 1874, t. lxxxii. p. 610; *Phil. Mag.* [4] vol. i. p. 492.

To control the accuracy of the mode of observation which we owe to MM. Fizeau and Foucault, we made a great number of experiments on the light of sodium by means of the very precise method made known by M. Mascart in his excellent paper "On the Modifications undergone by Light in consequence of the Motion of the Luminous Source" (*Ann. Sc. de l'Ecole Normale*, 1872, t. i. p. 202). Employing this method, which consists in operating on monochromatic light observed in a spectroscope with the slit very much widened, M. Mascart found  $21^{\circ}73$  for the rotation of the light of sodium at 15 degrees temperature. We arrived at the figure  $21^{\circ}727$  at the temperature of 20 degrees, which corresponds to  $21^{\circ}711$  at 15 degrees. This slight difference appears to us to come within the limit of the errors which this method permits when applied to light which is not strictly monochromatic.

On the other hand, we have made a great number of measurements for the solar lines  $D_1$  and  $D_2$  by Fizeau and Foucault's method. For  $D_1$ , by varying the conditions of observation, we obtained values between  $21^{\circ}750$  and  $21^{\circ}714$ : the mean of those in which we have most confidence is  $21^{\circ}736$  at 20 degrees. For  $D_2$  we have found  $21^{\circ}684$ . These numbers, between which falls the value obtained by M. Mascart's procedure, appear to us to prove the correctness as well as the delicacy of the method of MM. Fizeau and Foucault.

In making a first series of experiments the spectroscope we used was furnished with lenses of quartz and a prism of Iceland spar, with the fluorescent ocular for the observation of the ultra-violet rays\*, and with the ordinary eyepiece for a certain number of measurements taken in the luminous portion of the spectrum. In a second series of experiments we made use of ordinary lenses of optical glass, employing two flint-glass prisms for the lines comprised between A and G, and one prism only for the portion extending from h to M.

\* The spectroscope with fluorescent eyepiece, described by one of us, has well answered its purpose for the very refrangible rays from N to R. The solar light has to be reflected from a German-silver mirror (not a silver one, because silver absorbs the highly refrangible rays, as Prof. Stokes has already shown). To avoid the rotation which the collimator lens of the spectroscope impresses on the polarized rays when it consists of only one biconvex lens of quartz, a compensated lens is employed, formed of two plano-convex quartz lenses, perfectly equal, one with right-handed, the other with left-handed rotation, applied the one against the other by their plane faces. The Iceland-spar prism has its edges cut parallel to the axis; it therefore gives two spectra, of which the most deviating (the ordinary ray) is observed in preference. The substances which appear to suit best for the fluorescent layer are an aqueous solution of esculine for the portion of the spectrum from h to N, and a plate of uranium-glass for the rays of shorter wave-lengths: with this latter substance the lines R, S, and even T can be distinguished in the solar spectrum; and in the spectra of the metals the most refrangible lines can be seen—for instance, the twenty-fifth line of cadmium ( $\lambda = 221.7$ , Mascart).



The following Table sums up the results obtained \* :—

Lines of the spectrum.	$\lambda$ .	1st series.		2nd series.		Value of the angle of rotation.		
		Angle of rotation.	Number of observations.	Angle of rotation.	Number of observations.	Definitive.	Calculated.	Difference.
A .....	760.40	° .....	...	12.668	20	12.668	12.742	+0.074
a .....	718.36	14.293	40	14.304	20	14.304	14.337	+0.033
B .....	686.71	.....	...	15.746	20	15.746	15.746	0.000
C .....	656.21	.....	...	17.318	20	17.318	17.314	-0.004
D <sub>1</sub> .....	589.51	.....	...	21.684	20	21.684	21.689	+0.005
D <sub>2</sub> .....	588.91	21.729	16	21.736	40	21.736	adopted.	—
E .....	526.96	.....	...	27.543	20	27.543	27.530	-0.013
F .....	486.08	32.737	14	32.774	20	32.774	32.752	-0.022
G .....	430.72	.....	...	42.604	20	42.604	42.634	+0.030
h .....	410.12	47.518	50	47.481	20	47.499	47.514	-0.015
H <sub>1</sub> .....	396.76	51.208	16	51.178	20	51.193	51.151	-0.042
H <sub>2</sub> .....	393.29	.....	...	52.155	20	52.155	52.165	+0.010
L .....	381.96	55.712	20	55.539	20	55.625	55.701	+0.076
M .....	372.68	58.918	50	58.844	30	58.881	58.878	-0.003
N .....	358.05	64.459	30	.....	...	64.459	64.480	+0.021
O .....	343.97	70.585	30	.....	...	70.585	70.684	+0.099
P .....	336.02	74.574	20	.....	...	74.574	74.598	+0.024
Q .....	328.56	78.582	50	.....	...	78.582	adopted.	—
R .....	317.75	84.972	37	.....	...	84.972	84.960	-0.012

We have adopted as the definitive values the means of the observations of only the first series for the lines A to G, the means of both series for the lines from *h* to M, and the numbers of the second series from N to R. For broad lines and those which form part of a complex group, such as A, H<sub>1</sub>, H<sub>2</sub>, L, we reckon that these values are within 0°.1 of being exact; the approximation must be to 0°.05 for the other lines from *a* to N; the closeness then diminishes; but the deviation from accuracy does not exceed 0°.1 for R.

The values in the eighth column were calculated by the formula of Boltzmann,

$$\phi = \frac{B}{10^6 \lambda^2} + \frac{C}{10^{12} \lambda^4},$$

in which the values of the constants, deduced from those obtained by experiment for the lines D<sub>1</sub> and Q, are

$$B = 7.111540, \quad C = 0.148061.$$

\* The wave-lengths adopted, as set down in the second column of the Table, are those of Ångström from A to *h*, those of M. Cornu from H<sub>1</sub> to O, those of M. Mascart from P to R. The angles of rotation, in the third and fifth columns, in degrees and fractions of a degree, are the mean values of a number of observations, indicated in the fourth and sixth columns. The seventh column gives the values of the angles of rotation adopted as definitive; the eighth column, the same values calculated by Boltzmann's formula; the ninth column, the difference between the results of the calculation and those of experiment.

The ninth column of the Table gives the differences between the calculated and observed values.

M. A. Cornu has obliged us by placing at our service one of the specimens of quartz (No. 6) which had served M. Mascart for the determination of the rotatory power. It has given us results not sensibly differing from those which we had previously obtained\*. — *Comptes Rendus de l'Académie des Sciences*, tome lxxxiii. pp. 818–821.

#### ON THE RATIO OF THE TWO SPECIFIC HEATS OF A GAS.

BY CH. SIMON.

My purpose, in this communication, is to determine, by the theory, the ratio  $\frac{C}{c}$  of the two specific heats of a perfect gas, simple and tetratomic.

When a gas is regarded as a system of material points the mutual actions of which are negligible, the ratio  $\frac{C}{c}$  is readily found to be equal to  $\frac{5}{3}$ ; and M. Villarceau has proved that the result is the same when a mutual action is supposed between two gaseous molecules which is a function of their distance and directed along the right line that joins their centres of gravity; but this result has only been verified hitherto in the case of mercury vapour, which is considered to be monatomic. For any gas whatever, M. Villarceau† has given the expression

$$\frac{C}{c} = -\frac{2}{3} \frac{1}{Ec} \left( \frac{d\Sigma \Sigma \frac{1}{2} \mu w^2}{d\theta} + \Sigma \Sigma \phi \frac{d\delta}{d\theta} \right),$$

where the quantity in the brackets represents the variation, per degree of increase of temperature, of the energy of the system of the atoms in their motions about the centres of gravity of the molecules of which they are parts. This quantity consequently includes the internal vibrations and the rotations of the molecules. Ignorant as we are of the laws which govern the intramolecular forces, it would perhaps be impossible to reach by calculation the internal vibrations; but it appears easier to take into consideration the rotations of the molecules about their centres of gravity; and it is to be presumed that, if account be taken of the rotations, the internal

\* With this quartz we found, for the value of the rotation of the light of sodium, by Mascart's method,  $21^{\circ}.725$ ; and for different lines, by the method of Fizeau and Foucault, we arrived at the following numbers:—

A.	a.	D.	h.	M.	O.	P.	Q.	R.
12.677	14.319	21.750	47.509	58.900	70.538	74.622	78.623	85.017

† *Comptes Rendus*, May 22, 1876.

vibrations being neglected, we shall obtain, if not the exact value of the ratio  $\frac{C}{c}$ , at least a value more closely approximate than  $\frac{5}{3}$ .

That is the idea which has led me to undertake this research.

To calculate the *vis viva* due to the rotations of the molecules of a gas, recourse must necessarily be had to certain hypotheses. Without introducing any new ones, I adopt those which appear to me the most plausible and are most generally accepted. I call *perfect gases* those which follow the laws of Mariotte and Gay-Lussac, and in which the internal work is *nil*, so that their molecules have velocities of translation of which the squares are proportional to the absolute temperature. I call *simple* and *tetratomic* the gases whose molecules are composed of four smaller molecules identical with each other—not necessarily atoms, but which may be regarded as atoms in the present state of science: such are (or appear to be) hydrogen, oxygen, nitrogen, &c. I imagine that, in such a gas, the four atoms occupy the apices of a regular tetrahedron, the edge of which is longer than the diameter of each of them, and that the interior of the tetrahedron is filled with æther free or condensed. Now, taking account of the rotation of each elementary tetrahedron about its centre of gravity, and regarding the vibrations of the atoms as none or insensible, I have found the ratio of the two specific heats to be exactly  $\frac{7}{5}$  or 1.4, while experiment has given, for the gases above-mentioned, values between 1.39 and 1.42.

From this accordance between calculation and experiment it seems to follow that the internal vibrations, neglected in the calculation, are negligible in reality. I am thus led to think that, in simple gases, the physical molecules remain sensibly invariable in form and dimensions as long as no electrical or chemical action is produced.—*Comptes Rendus de l'Académie des Sciences*, tome lxxxiii. pp. 726–728.

#### A NEW PROCESS FOR THE SEPARATION OF GALLIUM.

BY LECOQ DE BOISBAUDRAN.

I have recently simplified and much shortened the preparation of gallium by operating as follows:—

1. The ore is dissolved, according to its nature, in aqua regia, chlorhydric acid, or sulphuric acid. The liquid is treated, cold, with plates of zinc, and then filtered while the liberation of hydrogen is still pretty considerable; the liquid is then heated with a large excess of zinc. The gelatinous precipitate is washed, and taken up again by chlorhydric acid. The new solution is heated with an excess of zinc, and a second gelatinous precipitate obtained. To this point the course is identical with that of the former process (*Comptes Rendus*, May 8, 1876, p. 1098).

2. Into the chlorhydric solution of the second precipitate formed

by the zinc a current of sulphuretted hydrogen is passed; it is filtered, the sulphuretted hydrogen expelled; and finally carbonate of soda is added fractionally until the line Ga  $\alpha$  417 ceases to be visible with the chlorhydric solution of the precipitate.

3. The oxides (or subsalts) are again taken up by sulphuric acid; the solution is cautiously evaporated until no more white sulphuric fumes (or scarcely any) are liberated. It is allowed to cool, then agitated with water, which dissolves the mass after a time varying from a few hours to two days. The solution of the nearly neutral sulphate is diluted with water and heated to *ebullition*, and the subsalt of gallium separated by filtration hot.

4. This basic salt is dissolved in a little sulphuric acid; and a slight excess of caustic potass is added, so as not to separate the gallium but to throw down the iron. Filtration follows, and then the passage of a prolonged current of carbonic acid gas, which precipitates the oxide of gallium.

5. This oxide is redissolved by the smallest possible quantity of sulphuric acid; a small excess of slightly acid acetate of ammonia is added, and then sulphuretted hydrogen is passed into the solution. Under these conditions gallium is not precipitated.

6. The acetic liquid is filtered, diluted with water, and brought to ebullition; the largest portion of the gallium is precipitated. It is filtered while hot. The mother-liquor, concentrated and boiled with aqua regia (in order to break up the salts of ammonia), is put with the other gallium-residues.

7. The precipitate formed by the hot acetic liquor is redissolved by sulphuric acid; we add a slight excess of caustic potass, and filter.

8. The potassic solution is electrolyzed. The metallic gallium is readily detached from the plate of platinum by squeezing this between the fingers under tepid water.

9. The metal is kept for about half an hour, at a temperature of  $60^{\circ}$  or  $70^{\circ}$  C., in nitric acid (*quite free from chlorine*) diluted with its volume of water; after washing, it may be considered pure.

The various galliferous residues from the manipulations nos. 2 to 9 are mixed, and treated first with fractional parts of carbonate of soda, to separate the greater portion of the zinc, aluminium, &c., and then with potass to remove the iron. The product is joined to that of operation no. 2. When we arrive at a residue containing but little gallium and much iron, the simplest way is to treat it with zinc, heating it and excluding the air; the greater part of the iron will remain in solution.—*Comptes Rendus de l'Académie des Sciences*, tome lxxxi. pp. 636–638.

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LX. *On Electricity disengaged between Mercury Surfaces; and on the Motions produced in Mercury by Deoxidation.*  
By ROBERT SABINE\*.

IN the spring of last year the late Sir C. Wheatstone had his attention directed to Erman's discovery, that a globe of mercury in contact with an electrolytic liquid within a tube of small calibre moved forward when a current of electricity was passed through it†. Taking advantage of this motion, Sir Charles invented a novel form of telegraph receiving-instrument which he described in a patent‡ taken out in the autumn, and in making experiments upon which he was engaged in France at the time of his death.

As one of his executors, the duty devolved upon me of ascertaining how far this novel telegraphic instrument could be made available for actual work; and to this end I found it necessary to undertake a series of preliminary experiments in order to obtain a better knowledge of the principles which underlie the phenomenon. Such an inquiry Sir Charles had in contemplation, and would, I believe, have carried out after his return to England. I shall shortly give an account of the performance of different forms of this receiver; but before doing so, I propose, in the following pages, to submit the experiments I have made, and which have not only revealed phenomena not observed before, but have suggested some explanations different from those offered by experimenters on the electrical behaviour of mercury.

A preliminary series of experiments led me to conclude that every mercurial surface, when exposed to the influence of air or water, consists of mercurous oxide and not of metallic mercury, that the electropositive condition of a

\* Communicated by the Author.

† Erman, *Gilb. Ann.* vol. xxxii. (1809), p. 261.

‡ No. 2771, dated Aug. 5, 1875.

mercury surface is greatly dependent upon its state of oxidation, that the expansion or contraction of such a surface is always attended with a corresponding alteration of its electropositeness, and, finally, that the motions of mercury electrodes, under water or acidulated water, are due principally to the reduction of superficial suboxide.

The following more systematic series of experiments was undertaken to ascertain the correctness of these conclusions.

### I. *Electricity disengaged between Mercury Surfaces.*

In a paper contributed in 1871 to the Royal Society\*, "On the Polarization of Metallic Surfaces in Aqueous Solutions," Mr. C. F. Varley made known some experiments by which he had succeeded in producing electric currents between two masses of mercury, the relative exposed surfaces of which, under dilute sulphuric acid, were varied by alternately rising and falling in two funnel-shaped vessels, the mercury surfaces having been previously polarized with hydrogen by connexion with the zinc pole of a battery. Mr. Varley, however, did not observe any current with unhydrogenized mercury when it was pure. In my experiments on the production of currents by mercury surfaces I have carefully avoided any hydrogenizing taking place, and have confined my attention to currents set up by the altered electropositive condition through oxidation by the atmosphere alone, or by simple contact with the liquid in which the mercury was immersed.


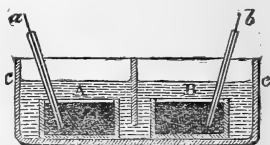
An insulating trough about 20 × 15 centims., and 7 centims. deep, was constructed with a division in the middle reaching to within a short distance of the bottom, and which divided it into two compartments (fig. 1). Distilled mercury was carefully poured into two dishes, A and B, one in each compartment, to the same level, whilst distilled water to the level *c* covered both the surfaces. Platinum conducting-wires (*a* and *b*) inserted through glass tubes enabled the two masses of mercury to be connected with a measuring-instrument. The trough was allowed to rest quietly for a time, until the temperature was settled; then one of the surfaces (A or B) was scraped. Two pieces of glass were taken, one in each hand, and placed upon the middle part of one of the mercury surfaces, thus . They were then quickly separated so as to cause a fresh mercury surface to be exposed; and at the same instant the release of a discharge-key, in connexion with an

Fig. 1.



\* Phil. Trans. vol. clxi. (1871), p. 129.

accumulator and measuring-galvanometer, allowed the electromotive force to be ascertained and compared with that of a standard cell.

The readings were found to depend greatly upon the energy with which the glass scrapers were parted, and upon the precision with which the discharge-key was released at the moment of scraping. The electromotive force between two surfaces of pure mercury which had stood a short time under distilled water was found to be as follows :—

A scraped, B quiet.	B scraped, A quiet.
0·033 volt.	—0·046 volt.
0·045    "	—0·020    "
0·041    "	—0·032    "

When very dilute sulphuric acid was substituted for the distilled water, the average readings became higher, as follows :—

A scraped, B quiet.	B scraped, A quiet.
0·067 volt.	—0·064 volt.
0·065    "	—0·086    "
0·032    "	—0·041    "
0·068    "	—0·077    "

The last experiment was varied by scraping one of the surfaces backwards and forwards with a glass scraper, and observing the current by a delicate mirror-galvanometer with a very high resistance in the circuit during the scraping, and at intervals of fifteen seconds after ceasing to scrape, until the two surfaces had resumed an equality electrically. Whilst the one surface was scraped, the other surface was at rest. The deflection showed in all cases that the scraped or disturbed surface corresponded with the zinc of a galvanic couple. It was also found that, however clean and bright the surfaces might appear, the scraping of one of them was always able to establish a current of electricity.

Surface A scraped gave a steady deflection of 130 divisions to the right, representing an electromotive force of 0·036 volt. When the scraping was discontinued, the deflection fell gradually as follows :—

After 15 seconds.....	90 divisions right.
"   30     "   .....	80     "
"   45     "   .....	50     "
"   60     "   .....	30     "
"   75     "   .....	20     "
"   90     "   .....	10     "

It was two minutes before the disturbed surface of the mercury had resumed a condition similar to that of the undisturbed.

Surface B scraped ; deflection = 185 divisions left, = 0.051 volt. Then left at rest.

After 15 seconds, deflection = 75 divisions left.

”	30	”	”	45	”
”	45	”	”	30	”
”	60	”	”	15	”
”	75	”	”	10	”
”	90	”	”	3	”

Instead of using a scraper, the point of a glass tube was next inserted in the mercury, and air blown through so as to keep its surface in continued motion and to break up any covering of suboxide. The electromotive force between the moving and quiet surfaces was then observed and similar results recorded.

In order to find whether this current might be due to thermoelectricity resulting from heat developed by the motion, the following experiment was made.

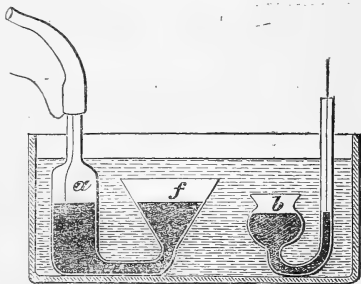
A vessel of heated mercury was provided, and, by means of a pipette, a quantity introduced into one of the compartments. The deflection consequent on the sudden change of temperature was observed as soon as it became steady. Heated mercury introduced into A. Temperature of A rose from 22°·5 C. to 27°·8 C. Temperature of B 22°·5 C. Deflection = 35 divisions right (of the same galvanometer). After an interval of ten minutes, heated mercury was introduced into B. Temperature of B rose from 24° C. to 29°·5 C. Temperature of A 23°·5 C. Deflection = 40 divisions left. It was evident that whilst a difference of temperature of 5°·5 C. occasioned only a deflection of about 40 divisions, a difference of 20° C. to 25° C. would have been required to produce effects equal to those observed through the scraping, whereas, during the whole of the previous readings, the temperature of the mercury had not varied in either of the dishes more than 1° C.; the currents were therefore not due to heat. The only other conclusion is that the metallic condition of the surface of the mercury is changed by the scraping, such as would result from a very thin film of mercurous oxide being pushed aside and exposing metallic mercury or a less oxidized surface to the action of the liquid. It is in the highest degree probable, indeed, that mercury, when exposed at ordinary temperatures to air or water, never presents a really metallic surface, or, if it ever does so, that this condi-



tion is of very short duration. The mirror-like brilliancy of a mercurial surface is no criterion that the whole of the visible molecules may not be suboxide; for the reflection would not be lessened until the formation of this suboxide became sufficient to impair the smoothness of the surface. And the currents measured between the disturbed and quiescent surfaces of mercury are probably currents set up between surfaces in different states of oxidation, the less oxidized of the two being positive to the other.

To put this more fully to the test of experiment, an apparatus was made up very much on the principle of that used by Mr. Varley for producing currents by mercury when polarized with hydrogen. The object in view was, that one of the surfaces of mercury might

Fig. 2.



be expanded or contracted at pleasure to either condense or break the supposed film of suboxide. The funnel-shaped vessel *f* (fig. 2) was connected with a reservoir of mercury, *a*, in such a way that, by increasing or decreasing the pneumatic pressure in *a*, the mercury in *f* would rise or fall. This vessel was immersed in a deep trough containing distilled water. The surface of the mercury in *f* formed one side of a galvanic couple, with a quiescent surface of mercury contained in the bent-up end of a thistle-tube *b* for its other side. When the mercury-niveau was down in the shank of the funnel, whatever suboxide might be upon it would be split up when the mercury was raised suddenly in the funnel. When contracted at the bottom of the funnel, the diameter of the mercury face was about 0.6 centim.; when raised and expanded, it was between 2 and 2.5 centims., the area being therefore 10 to 15 times as great.

When the temperature of the whole had become uniform, a very slight current was observed on the galvanometer connected between protected wires dipping into the two masses of mercury. The mercury was then drawn steadily down into the shank of the funnel, and a considerable deflection towards the right observed, showing a diminished electropositiveness. When the needle became still, the mercury was allowed to return to its former level; and an excursion to the left was noted. When the needle was again steady,

the mercury was caused to rise to the top of the funnel, when an excursion to the left was again observed\*.

A series of observations were then made, taking the readings of the steady deflections when the mercury was at the top and at the bottom of the funnel alternately, the difference between two consecutive readings being proportional to the increase or decrease of electromotive force between the surfaces.

Change of Deflection after the mercury surface,  $f$ , had been

Increased.		Decreased.
172 divisions.		-227 divisions.
178     "		-198     "
136     "		-162     "
137     "		-123     "
140     "		-115     "
116     "		-130     "
143     "		-121     "
178     "		-132     "
163     "		-171     "

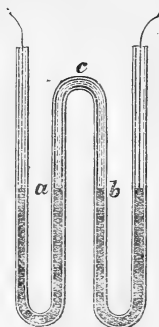
The mean of the two series represented a difference of electromotive force equivalent to 0.115 volt. When a small quantity of sulphuric acid was added to the water, similar results were obtained. When the acid water was strong, the effect was much sooner over. This was probably due to the combination of the acid with the mercurous oxide as the latter was formed.

It was observed that the change in the electromotive force took place more rapidly while the mercury surface was expanding than while it was contracting. While expanding, the light-spot invariably made an excursion far beyond the point to which it finally settled, whereas while the mercury was being contracted the light moved steadily up to its position of rest and swung only very few degrees beyond it,—indicating that the film of suboxide, while contracting, underwent a tolerably regular thickening; but when expanding, the film, not being elastic, split and suddenly exposed a surface of metallic mercury. The expanded or disturbed surface was always positive to the still surface; that is, it took the place of the zinc in a voltaic couple †.

\* If instead of letting the mercury rise from the bottom its surface was scraped, a deflection to the left was observed.

† This effect is the reverse of that said to be observed when the mercury surfaces are polarized with hydrogen. Mr. Varley says, "During this change of surface-dimensions [of mercury previously hydrogenized] currents of electricity will be found to pass from one to the other, the diminishing surface acting as the zinc plate, and the increasing surface as the copper plate of a voltaic couple."

A W-tube, 0.8 centim. diam., was made up as in fig. 3; mercury in the two lower parts, acidulated water in the junction *c*, and platinum wires in the outer limbs. On tilting the tube, so that the surface in *a* advanced and that in *b* receded, the mercury in *a*, by reason of the slight friction, flowed, of course, less readily in contact with the glass than at the centre; and so fresh surface broke through. On the other side (*b*) just the reverse took place; whatever suboxide was on the sides closed in and thickened.



The needle of the galvanometer between the platinum wires was deflected in one direction when the system was inclined one way, and when it was inclined the other way, in the other direction. The advancing surface brightened and was positive to the receding surface, which became duller.

Tube inclined from		Excursion of light-point.	
Upright	to left .....	+125	divisions.
"	" right .....	-125	"
Right	" left .....	+185	"
Left	" right .....	-187	"
Upright	" left .....	+118	"
"	" right .....	-115	"
Right	" left .....	+180	"
Left	" right .....	-175	"

The same was done with capillary tubes, and the effect found to be still more striking, whether the tube was tilted or the mercury caused to travel along by increasing the pressure of the air above one of the ends.

Then a straight length of capillary tube turned up at the ends and attached to mouth-pieces was filled with mercury, in the middle of it being a small quantity of acid water. When this was tilted or otherwise so that the acid water travelled along, an electromotive force was obtained equal to nearly one third of a Daniell element.

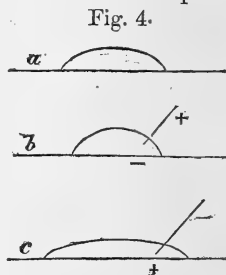
An ordinary U-tube had mercury poured into the bend to about an inch up each limb; acid water over this, and platinum wires in the acid water. When at rest, a small constant electromotive force was observed. When tilted, results precisely similar to those with the W-tube were found. With capillary tubes provided with enlarged ends, the effects were the same, and due unquestionably to the same cause.

Mr. Gabriel Lippmann\*, in his paper "On the Connexion between Capillary and Electrical Phenomena," ascribes the electricity which he observed to be disengaged by a mercury surface in acid water, advancing in a capillary tube, to a change in its capillary constant, without, however, adducing any collateral evidence. It appears to me that the electricity in question may be accounted for by different states of oxidation of the advancing and receding surfaces. So far, I have not found a single experiment to indicate this assumed connexion between capillary constant and disengaged electricity. That its oxidized state modifies the surface-tension of mercury I will show further on; that it also modifies the electropositive state of the surface is evident from the foregoing; and two things, both which depend upon a third, might have some proportionality between them. But there does not appear to be any nearer connexion, if this, between electricity and capillarity of mercury; and it is, I venture to suggest, premature to call a difference of electrical potential produced in this way, "polarization by capillary forces," until at least the "capillary forces" in question have been more closely interrogated.

## II. Motions produced in Mercury by Deoxidation.

The following may be taken as representative of a class of similar phenomena which have been made known by different observers. When mercury is poured into a flat dinner-plate to a depth of a quarter of an inch, and a drop of water placed upon it, the drop, as is well known, assumes an arched section (*a*, fig. 4). If now the mercury be connected with the zinc pole and the drop by means of a fine platinum wire with the copper pole of a battery of two or three elements, the drop will contract and become more spherical, as in *b*. When the battery-circuit is interrupted, the drop of water resumes its original form (*a*). If the battery-current be reversed so that the copper pole is connected with the mercury and the zinc pole with the drop, the latter gradually expands as in *c*, whilst the mercury surface seen through the water becomes clouded with a slight film of suboxide. Reversing again the battery, destroys the film and contracts the drop.

Two views have been hitherto accepted by physicists as explanatory of these phenomena. The first is that given by Erman in his paper already cited, viz. that the reduction of



\* Pogg. *Ann.* vol. cxlix. p. 546; *Phil. Mag.* [IV.] vol. xlvii. p. 281.

the superficial suboxide by the current, producing a cleaner metallic surface, decreases the adhesion of the water, and this causes the drop to contract; and in the other direction, that the current oxidizing the surface increases the adhesion of the water, which spreads out in consequence. The second explanation is that suggested by Dr. J. W. Draper, in 1845, in a paper\* entitled "Is Capillary Attraction an Electrical Phenomenon?" According to his view, the capillary constant at the contact-surface of mercury and dilute sulphuric acid is supposed to be altered by the current; and hence the contraction or expansion, the surface of mercury behaving like an elastic membrane. It appears to me that both adhesion and capillarity play very insignificant parts in the phenomena in question. The explanation which I offer is that these contractions and expansions are due simply to the different atomic volumes of mercury and its suboxide.

If small particles of dry solid matter be placed upon the bright surface of the mercury just outside the water-drop, before connecting the battery, their distances from the periphery of the drop remain practically unchanged both when the drop contracts and when it expands. From this it is obvious that the expansion of the water-drop is not due to its overrunning fresh mercury surface, which would correspond with an increased adhesion, but simply to the expansion of the mercurial area originally covered by the water—and that the contraction is not a retiring from the mercury surface, such as would result from diminished adhesion, but a contraction of the covered surface. In other words, the water is the index of the contraction or expansion of a certain area of the mercury which is undergoing some chemical change. When the water-drop is placed upon the mercury surface, that surface is already (or immediately afterwards becomes) converted into suboxide. When the drop is connected with the battery so that the suboxide is reduced, the base of the drop becomes metallic and necessarily contracts to an area corresponding with the reduced atomic volume of the molecules which form the surface. On the other hand, when the battery-current is broken, the metallic mercury surface, sensitive to oxidation, is ready to return at once to its state of suboxide and resumes its original dimensions. And, finally, when the battery-current is reversed so that the mercurial surface underneath the drop is still further oxidized, the drop will go on expanding in consequence of the formation of new oxide underneath that first formed, the oxide being porous and allowing the water to percolate through it.

\* Draper, *Phil. Mag.* May 1845, p. 185.

Viewed in this light, the water-drop may be said to perform two distinct functions:—(1) that of determining the area of mercury the visible molecules of which are to be changed from metal to suboxide, and *vice versâ*; (2) that of showing to what extent the covered mercury surface is expanded or contracted in consequence.

And if this view is correct, it follows that, by knowing the relative atomic volumes of mercury and its suboxide, we could predict approximately to what extent any given water-drop on a common surface of mercury would be contracted by the reducing current.

The atomic weight of mercury is 200, whilst its specific gravity is 13·595; therefore its atomic volume is

$$\frac{200}{13\cdot595} = 14\cdot7.$$

The atomic weight of suboxide of mercury is 416. Its specific gravity is not so well ascertained. Different authorities assign it different values:—Gerstenhöfer \* gives 12·07; Herapath found it 10·69, and Karsten 8·95†. I assume that Herapath's value is the most trustworthy, viz. 10·69, which is nearly a mean of the other two. The atomic volume of suboxide of mercury would therefore be

$$\frac{416}{10\cdot69} = 38\cdot9.$$

Now if the diameter of the water-drop when first placed upon the mercury is  $d$ , and it contracts to  $d'$  under the action of the reducing current, it is evident that we should find

$$\frac{d}{d'} = \sqrt[3]{\frac{38\cdot9}{2 \times 14\cdot7}} = 1\cdot098;$$

that is to say, we should be prepared to find the water-drop about 10 per cent. larger in diameter when first placed upon the mercury than when the latter is put under the reducing action of the battery.

The following series of measurements with different drops of distilled water was made before and after deoxidation of the mercury.

\* *Hilfsbuch für den Gewerb. Chemiker* (1851), p. 45.

† Watts's 'Dictionary of Chemistry' (1875), vol. iii. p. 907.

Measured diameter of drop.		
At rest.	Contracted.	
$d$ .	$d'$ .	$\frac{d}{d'}$ .
3.35 centims.	3.1 centims.	1.08
3.4 "	3.0 "	1.13
3.45 "	3.2 "	1.08
3.55 "	3.35 "	1.06
2.9 "	2.7 "	1.07
3.7 "	3.45 "	1.07
4.0 "	3.6 "	1.11
3.6 "	3.3 "	1.09
3.4 "	3.15 "	1.08
3.5 "	3.25 "	1.08
3.85 "	3.6 "	1.07
4.15 "	3.8 "	1.09
3.35 "	3.1 "	1.08
3.75 "	3.5 "	1.07
Mean . .		1.08

With dilute sulphuric acid with one Smee cell.

2.2 centims.	2.05 centims.	1.07
2.35 "	2.1 "	1.12
2.4 "	2.1 "	1.14
2.5 "	2.3 "	1.09
2.3 "	2.1 "	1.10
2.3 "	2.0 "	1.15
2.25 "	1.95 "	1.15
2.1 "	1.9 "	1.10
2.55 "	2.3 "	1.11
2.2 "	2.0 "	1.10
2.4 "	2.25 "	1.07
2.25 "	2.05 "	1.10
Mean . .		1.11

The mean with acidulated water is higher than that with distilled water. This may be due to the greater conductivity of the acid water causing the reducing action to be more complete and prompt, and partly to reduction of sulphate.

It has been shown by Mr. Paalzow\* that a drop of water placed upon mercury contracts as soon as a small crystal of sodium hyposulphite is dropped into it. The action is in this case identical with that of the reducing current of the battery. The following measurements of the diameters of water-drops

\* Pogg. Ann. vol. civ. (1858), p. 420.

on mercury were made before and after deoxidation by this means :—

$d.$	$d'.$	$\frac{d}{d'}.$
1.85 centim.	1.7 centim.	1.09
2.0 centims.	1.75 „	1.14
1.95 centim.	1.8 „	1.08
2.4 centims.	2.19 centims.	1.10
2.73 „	2.52 „	1.08
3.83 „	3.55 „	1.08
Mean . .		1.095

Allowing for the difficulty of measuring liquid drops\*, which are not always quite circular, and for a cause of error arising from circulation in the mercury, these values are confirmatory of the view I have stated. With greater care and better means of measuring the area of the drop, and of regulating the tension of the mercury surface outside the drop, these values would, I have no doubt, be found to correspond strictly with the changed atomic volume of the molecules at the base, and might even be employed as a means of deducing the specific gravity of the suboxide, which is at present rather difficult on account of its instability.

When a battery of higher power is used for the deoxidation of the mercury, the drop of water or acid water is observed at first to contract and soon afterwards slightly to expand again. On closely observing the water, its interior is seen in rapid circulation; and fixing the attention upon any little floating particles, it is seen that close to the mercury they travel away from the immersed positive wire for a short distance, then rise up towards the surface, and return in the other direction. The distance between a travelling particle and its reflected image in the mercury allows the direction of circulation to be very exactly observed. This circulation, which is communicated to the water-drop by a similar circulation in the mercury underneath, causes the drop to become slightly expanded. Therefore whenever the battery is strong enough to establish any circulation, the diameter of the drop cannot be found to correspond with its deoxidized state. This source of error is difficult altogether to avoid.

In order to be able better to observe the effect of this circulation, which appeared to be due to unequal chemical action on the surface of the mercury, the following experiment was arranged :—

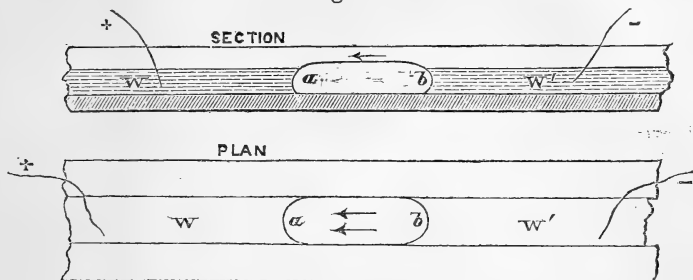
A glass trough was constructed with a groove about 0.6

\* Photography suggested itself for this purpose.



centim. wide, 0.6 centim. deep, and 10 centims. long. In the middle of it was placed a globule of mercury, *ab* (fig. 5);

Fig. 5.



and on each side of the mercury the spaces, *w* and *w'*, were about half filled with water. Two wires, + and —, dipped into the water near the ends, and were connected with a battery. The object in view was to oxidize one portion of the surface of the mercury globule whilst another portion of it was being deoxidized. When the battery was connected so that the current passed through the trough from + to —, it is evident that the surface *a* of the mercury was deoxidized, whilst at the same time the opposite surface *b* of the globule was oxidized. The result of this double action it was easy to foresee. The surface *a* becoming deoxidized, contracted, whilst the surface *b*, becoming more oxidized, expanded.

The upper surface of the mercury globule was thereby thrown forwards, in the direction shown by the arrows, carrying with it more oxidized surface, which was promptly reduced on its arrival at *a*. The whole mercury globule was set in rapid circulation, the end *b* supplying the oxidized and expanding skin as fast as the end *a* could reduce and contract it. When the water was slightly acidulated, this passage of the oxidized surface became so vehement that, the globule being retained in its place, the water from *w'* was carried mechanically over and heaped up on the side *w*. But when the mercury globule was free to move, it marched bodily forwards from *w* to *w'* (opposite to the arrows) rather quickly towards the negative wire of the battery—that is, in the same direction in which the electricity is assumed to flow.

At the time the upper surface of the mercury globule was speeding along, the under surface was also observed to circulate in the same direction; but on account of friction offered by the trough, its motion there appeared to be slower. The interior circulation of the globule appeared to be approximately that shown by the arrows in fig. 6—*b* being the ex-

panding surface where the suboxide was formed, and *a* the contracting surface where it was reduced. The motions of its surface were observed by slightly inclining the trough, which prevented the mercury travelling. When the negative wire was plunged into the mercury globule instead of into the water, so as to leave the process of oxidation to the simple contact between the mercury and the acid water, similar motions were observed, but not so energetic. With the oxidation of the surface *b* in acidulated water, a quantity of insoluble sulphate was formed and carried mechanically over to *a*, where, after a time, it accumulated sufficiently to form a shell which remained perfectly still, covering the end *a*, whilst the end *b* was in active circulation. This shell increasing gradually, at last covered the whole globule, which nevertheless was found to be circulating actively beneath it. The appearance of this sulphate shell is very deceptive, as one is inclined sometimes to conclude that because it is at rest, the mercury underneath must be at rest also, which is not the case.

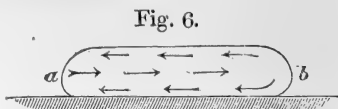
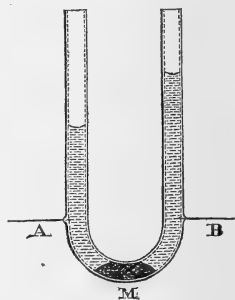


Fig. 6.

The mechanical effect produced by the rapid circulation of mercury found in the last experiment was also observed as follows:—The globule of mercury, instead of being placed in a horizontal trough, was deposited at the bottom (M) of the bend of a U-tube (fig. 7) and acid water poured into each limb above.

Fig. 7.



The quantity of mercury was just sufficient to form a valve, that is, to prevent communication between the acid water in the two limbs, but not much more. Two platinum wires, connected with a reversing-key and five Smee cells, dipped into the acid water in the limbs. When the circuit was closed, the current, going from A to B, began to oxidize the mercury face in B, and to deoxidize that in A. The mercury was at once set in rapid circulation and mechanically carried the water from limb B into limb A until the different heights of the columns of liquid balanced the pressure of the circulating mercury. The battery-circuit was then broken; and the acid water remained at its unequal level, the mercury acting as a valve. Then the direction of the current was reversed and circulation of the mercury commenced in the opposite direction, carrying the acid water back with it from A and heaping it up in B as in the figure.

Thus, by changing the direction of the battery, the electrolytic liquid could be transferred at pleasure from one limb to the other, rising on that side in which the positive wire was inserted, whilst the mercury globule never moved more than  $\frac{1}{8}$  inch from its original position.

With a U-tube of 1·3 centim. bore, the acid water attained a difference of level of 1·8 centim. when no resistance was inserted in the circuit, and the *niveau* rose 1·3 centim. in twenty seconds.

With a U-tube 0·6 centim. diameter, the acid water attained the following differences of level :—

Resistance inserted. ohms.	Difference of level. centim.
0 .....	2·27
1000 .....	2·05
5000 .....	1·60
10000 .....	1·30

The times (noted with a stop-watch) which the column took to travel from 0·5 centim. below to 0·5 centim. above the zero of the scale were as follows :—

Resistance inserted. ohms.	Time taken to alter level 1 centim.
0 .....	4·6 seconds.
1000 .....	5·9     ,,
5000 .....	16·2     ,,
10000 .....	25·5     ,,

From this, the rate at which the liquid is pumped by the circulating mercury drop appeared to be almost proportional to the current.

When the quantity of mercury in the U-tube was increased so that its level was 2 or 3 inches up the tube, the quantity of acid water which it pumped was extremely small, on account of the increased resistance of the passage between the mercury column and the glass. But instead of this, the level of the mercury in the two sides of the U-tube was altered, rising on that side in which the negative wire was plunged until the altered level balanced the force exerted by the circulation of the mercury.

This brings us to the experiment published by Draper (1845), in which he employed a U-tube, one limb of which was of larger diameter than the other, the narrower one only containing acid water above the mercury. With this apparatus Draper observed the alteration of level in question when the current was sent through ; and he ascribed the phenomenon to a supposed alteration in the capillary constant of the mercury. A modification of Draper's apparatus was intro-

duced in 1873 by Mr. G. Lippmann under the name of a "capillary electrometer," in which similar circulations set up in the mercury produce, of course, a similar alteration of level, but in explanation of which Mr. Lippmann reproduces Draper's theory of capillarity. In another form of Mr. Lippmann's "electrometer," a column of mercury is supported vertically in a tube, the bottom of which is drawn out to a capillary bore and dips into dilute sulphuric acid. When the capillary *niveau* is deoxidized, the circulation which is set up in the mercury tends to cause it to recede. This recession is opposed and finally balanced by the superincumbent column. When the capillary end of the tube is slightly conical, which is generally the case, it is observed that on breaking the circuit the *niveau* of the mercury returns to very nearly its original position. These motions require a very small current to produce them, the different states of oxidation of the two mercury surfaces, when the circuit is simply closed without a battery, being generally sufficient to cause a perceptible movement.

The movement of the mercury in Draper's experiment was limited by the weight of the column raised by it. When a straight horizontal tube (capillary or otherwise) provided with enlarged ends is used, the acid water in it being connected with the battery, the mercury globule pumps very little water past it, but marches itself onwards to the end of the tube. With a tube whose bore was about 0.05 centim. diameter, Sir C. Wheatstone found that a single Daniell cell caused sufficient motion to take place, when 300,000 ohms resistance were inserted in the circuit, to give readable signals\*.

If an inequality could, in any way, be produced between the capillary constant at two points of a mass of mercury, without being due to an alteration of the chemical constitution of the surface, such an inequality might set the mercury in movement. But by no means that I am aware of can the capillary constant of mercury, as long as the chemical nature of its surface continues unchanged, be made inconstant. To a common cause, displacement by chemical action, are due both change of surface-tension and locomotion; and the one effect cannot, in any sense of the word, be said to be the cause of the other.

\* This experiment is, in effect, the same as that devised by Erman (in 1809), which first suggested to Sir C. Wheatstone the idea of constructing a mercury receiving instrument for telegraphic purposes. Erman offers the following explanation of his experiment:—"The elongation and progressive motion of the cylinder of mercury in the tube may be strictly explained by the mere augmentation of adhesion and by the introduction of the water into the capillary space which results from it." Erman also suspected "an attraction acting at a distance."

To find the effect exerted upon the capillary constant of a common mercurial surface by deoxidation and further oxidation, the following experiments were made:—

A copper wire of 0·3 centim. diameter and 18 centims. long was furnished with a disk 1·7 centim. diameter soldered upon one end. The lower part of the wire and upper side of the disk were varnished, and the under side of the disk kept carefully amalgamated for some days in order to avoid any change in the degree of amalgamation taking place during the experiments; at its other end, the wire was suspended and centred from the beam of a chemical balance. A vessel of distilled water was then placed under it, the water reaching about 1 centim. over the disk. In this position counterweights were put into the scale-pan to balance it. Then mercury was introduced into the vessel until it reached and made contact with the amalgamated face of the disk, some of the water being removed to bring it to its original level. Weights were then added to the scale-pan to detach the amalgamated disk from the mercury—that is, to find what column of mercury the outer skin was able to sustain. A similar series was also made with acid water. A battery was then connected between the mercury in the vessel and a platinum wire immersed in the water. The resultant electromotive force of the circuit was measured from time to time to avoid uncertainty through polarization.

*Mercury being deoxidized.*

Liquid.		Detaching weight.	grms.
Distilled water.	Circuit open.....	{ 9·56 9·58 }	mean 9·57
	Electromotive force =	{ 9·65 9·65 }	9·65
	1·2 Daniell	{ 9·64 9·62 }	9·63
	5·5 "	{ 9·65 9·65 }	9·65
	11·0 "	{ 9·58 9·58 }	9·58
	Circuit open.....	{ 9·81 9·83 }	9·82
	Electromotive force =	{ 9·83 9·83 }	9·83
Dilute sulphuric acid	0·47 Daniell	{ 9·82 9·84 }	9·83
	" 0·81 "	{ 9·82 9·84 }	9·83
	" 1·74 "	{ 9·83 9·83 }	9·83
		{ 9·83 9·83 }	9·83

With each battery-power the deoxidation was continued during two or three hours, the readings being taken at intervals. Weighings with higher battery-powers were impracticable, on account of the adhesion of hydrogen bubbles around the suspended disk.

It appears from the above experiments that under water and acidulated water the surface-tension of mercury is slightly increased when deoxidized by an electric current, the deoxidation under acid water appearing to be more complete than under distilled water. When the mercurial surface was deoxidized by means of sodium hyposulphite, the effect was similar to that produced by the battery with acid water; but the deoxidation did not appear to be so complete.

The amalgamated copper disk of the previous experiments was balanced as before; and the weights required to detach it from a surface of mercury in (1) distilled water, and (2) when crystals of hyposulphite of soda were dissolved in it, were measured, as follows:—

Liquid.	Detaching weight.	
(1) Distilled water.....	$\left\{ \begin{array}{l} 9.58 \\ 9.57 \\ 9.59 \end{array} \right\}$	mean 9.58 grms.
(2) Crystals of sodium hyposulphite added .....	$\left\{ \begin{array}{l} 9.80 \\ 9.78 \\ 9.79 \end{array} \right\}$	„ 9.79 „

*Mercury being oxidized.*

When a mercury surface is undergoing a process of oxidation by the battery, it is, as we know, expanding correspondingly with the atomic volume of the metal and that of its suboxide. We are therefore prepared to find a greatly reduced detaching weight, due, not to any change in the capillary constant of mercury properly so called, but to an actual destruction of the surface itself.

Measurements were made with the same amalgamated disk whilst the mercury was being oxidized both under water and under dilute sulphuric acid, with the following results:—

Liquid.		Detaching weight.	grms.
	Circuit open .....		9.59
Distilled water	Electromotive force =	$\left\{ \begin{array}{l} 9.56 \\ 9.58 \end{array} \right\}$	mean 9.57
	1.0 Daniell	$\left\{ \begin{array}{l} 9.55 \\ 9.55 \end{array} \right\}$	„ 9.55
	„ 6.2 „	$\left\{ \begin{array}{l} 9.52 \\ 9.52 \end{array} \right\}$	„ 9.52
	„ 11.4 „	$\left\{ \begin{array}{l} 9.52 \\ 9.52 \end{array} \right\}$	„ 9.52
Dilute sulphuric acid.	„ 0.4 „	.....	8.95
	„ 0.68 „	.....	8.45
	„ 1.68 „	.....	7.08

If at any moment the electric circuit was interrupted, the weight required to detach the disk immediately increased, showing that the relaxation of surface-tension was due to the process of oxidation, and not to the presence of oxide only.

It is evident from the foregoing experiments:—(1) that the deoxidation of an ordinary surface of mercury to a slight extent increases its surface-tension; but when the deoxidation is as complete as it can become in the liquid employed, no further change appears to take place by increasing the battery-power; and (2) that with the mercury in process of oxidation we have to deal with a surface which is being transformed into a substance with little or no cohesion, at a rate increasing with a stronger current.

In this investigation I have confined myself to some of the direct effects of deoxidation of mercurial surfaces. There are other well-known phenomena of the motions of mercury in which the reduction of other metals from solutions plays an important part; but they are beyond the purpose of the present inquiry.

To sum up the results, we are, I think, entitled to conclude:—

(1) That the electricity disengaged between pure mercury surfaces (when not hydrogenized) is due to different states of oxidation.

(2) That the motions observed to take place in mercury electrodes under water (or dilute sulphuric acid) are due to the displacement of the surface corresponding to the different atomic volumes of the metal and its suboxide (or suboxide and sulphate).

(3) That the pumping action of a column of mercury in a tube or narrow channel is due to rapid circulation caused by such displacement.

(4) That these motions are not due to change of either constant of capillarity or adhesion. And, finally,

(5) That we may give a negative answer to Draper's question, viz. "*Is capillary attraction an electrical phenomenon?*"

In these experiments I have had the advantage of the cooperation and assistance of Mr. M'Eniry, who also carried out all the experiments which the late Sir C. Wheatstone made on the subject of his mercury receiver.

London, October 6, 1876.

LXI. *Researches in Acoustics.*

By ALFRED M. MAYER \*.—No. VIII.

[Continued from 4th Series, vol. xlix. p. 432.]

## CONTENTS.

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4. Applications of the Interferences of Sonorous Sensations to Determinations of the Relative Intensities of Sounds.

THIS communication is preliminary to an elaborate paper on the above subjects. For conciseness and clearness I present the few facts I have now to offer in the form of notes of experiments.

1. *On the Obliteration of the Sensation of one Sound by the simultaneous action on the ear of another more intense and lower sound.*

*Experimental Observations on the Obliteration of one Sound by another.*—Several feet from the ear I placed one of those loud-ticking spring-balance American clocks which make four beats in a second. Then I brought quite close to my ear a watch (made by Lange, of Dresden) ticking five times in the second. In this position I heard all the ticks of the watch, even those which coincided with every fourth tick of the clock. Let us call the fifth tick of the watch which coincided with one of the ticks of the clock, its fifth tick. I now gradually removed the watch from the ear and perceived that the fifth tick became fainter and fainter, till at a certain distance it entirely vanished and was, so to speak, “stamped out” of the watch †.

Similar and more striking experiments were made with an

\* Communicated by the Author.

† In the publication of this paper in ‘Nature,’ Aug. 10, 1876, my friend Mr. Alexander J. Ellis, F.R.S., appends the following note to the above experiment:—“The precise numbers of ticks in a second here mentioned are not necessary for roughly observing and understanding these phenomena. I observed them by a common American pendulum-clock placed on a table (which increased the power of its half-second ticks), and a watch beating five times in two seconds. The Rev. Mr. Haweis informs me that he has often noticed a similar effect at night with ordinary watches. The sensation produced by the obliteration of the tick when the proper distance of the watch from the ear has been attained, and the consequent sudden division of the ticks into periods separated by silences, is very peculiar. It is difficult not to believe that some accident has suddenly interfered with the action of the watch instead of merely with our own sensations.”—A. J. E.



old silver watch, beating four times to the second, by causing this watch to gain about thirty seconds an hour on the clock, so that at every two minutes the ticks of the watch and clock exactly coincided. When the watch was held near the ear, every one of its ticks was heard distinctly; but on gradually removing it from the ear the ticks of the watch became fainter and fainter at the coincidences, and when the watch had been removed to a distance of nine inches from the ear the ticks of the watch were utterly obliterated during *three* whole seconds of its ticks about the time of coincidence. On removing the watch to a distance of twenty-four inches I found that I lost its ticks during *nine* seconds about the time of coincidence. It is here important to remark that the ticks of the clock are *longer* in duration, as well as *lower* in pitch, than those of the watch. With the watch remaining at a distance of twenty-four inches from the ear, I listened with all my attention as tick by tick the watch approached the time of coincidence. Since the ticks of the watch are shorter in duration than those of the clock, they are *overlapped* by the others about the time of coincidence. Hence as, so to speak, the short ticks of the watch glided tick after tick under the long ticks of the clock, I perceived that more and more of the duration of each successive watch-tick became extinguished by the tick of the clock, until only the *tail* end of the short tick of the watch was left audible; and at last even this also crept under the long tick of the clock, and the whole of the ticks of the watch were rendered inaudible for *nine* seconds, at the end of which time the front or *head* of the watch-tick, as we may call it, protruded beyond the clock-tick, and then slowly grew up into a complete watch-tick as before. In this succession of events the tick of the old silver watch (made by Tobias) disappears with a sharp *chirp* like a cricket's, and reappears with a sound like that made by a boy's marble falling upon others in his pocket. By this experiment, therefore, a gradual analysis is made of the effect of the tick of the clock on the tick of the watch, affording a beautiful illustration of the fact that one sonorous sensation may overcome and obliterate another.

*Experiments to determine the relative Intensity of the Clock-ticks which obliterate the Watch-ticks.*—The clock was placed on a post in the middle of an open level field in the country on nights when the air was calm and noiseless. The ticks of the clock became just inaudible when my ear was removed to a distance of 350 feet. The ticks of the watch became just inaudible at a distance of twenty feet. The ratio of the squares of these numbers makes the ticks of the clock about 300 times

as intense as those of the watch. On the same nights that I made the above determinations I also put the clock on the post, and placing against my zygomatic process a slender stick graduated to inches and tenths, I stood with my ear at distances from the clock of from eight to sixteen feet, and then slid the watch above and along the stick (taking care that it did not touch it) until it reached such a distance from the ear that its fifth tick just disappeared. Knowing the relative intensities of the ticks of clock and watch when placed at the same distance from the ear, the law of the reciprocals of the squares gives the relative intensities when the clock and watch are at the several distances obtained in the above experiments. Large numbers of such experiments have been made; and the results agree perfectly well when we take into consideration, first, the difficulty thrown in the path of the determinations by the *gradual* fading away of the watch-ticks as they approach coincidence with the clock-ticks, and secondly, the impossibility of arriving at *any* result at all if the slightest noise (the rustle of a gentle breeze, the piping of frogs, the bark of a distant dog) should fall on the ear of the observer when engaged in making an experiment. The general result of the numerous experiments thus made shows that the sensation of the watch-tick is obliterated by a coincident tick of the clock when the intensity of the clock-tick is *three times* that of the watch-tick. This result, however, must be regarded as merely approximative, not only from the manner in which it was obtained, but from the *complexity* of the sounds on which the experiments were made. It is interesting, however, both as being, I believe, the first determination of this kind that has ever been made, and as having opened out a new and important field of research in physiological acoustics.

*Experiments on the Interference of the Sensations of Musical Sounds.*—Reserving the further development of my discoveries for future papers, I will now briefly describe some of the more prominent and simple phenomena which I discovered in experimenting with *musical sounds*. At the outset I will remove an objection always made by those versed in acoustics but unacquainted with these new phenomena. It is as follows:—"You say that one sound may obliterate the sensation of another; but are you sure that the real fact is not an alteration of the *quality* of the more intense sound by the action of the concurrent feebler vibration?" I exclude this objection by experimenting as follows:—An open or closed organ-pipe is sounded forcibly; and at a few feet from it is placed the instrument emitting the sound to be oblite-

rated, which may be either a tuning-fork on its resonance-box or a closed organ-pipe communicating with a separate bellows. Suppose that in the following experiment both tuning-fork and closed organ-pipe produce a note higher in pitch than the more intense or extinguishing sound of the open organ-pipe. Now sound the fork alone strongly, and alternately shut and open its resonance box with the hand. We can thus obtain the sound of the fork in a *regular measure of time*. When the ear has well apprehended the intervals of silence and of sound thus produced, begin the experiment by sounding the open pipe and tuning-fork simultaneously. Now if any change is thus effected in the quality of sound emitted by the open pipe, this change cannot occur except when the fork is sounded, and hence, if it occurs at all, it must occur in the regular measure in which the fork is sounded. The following are the facts really observed. At first every time that the mouth of the box is open the sound of the fork is distinctly heard and changes the quality of the note of the open pipe. But as the vibrations of the fork run down in amplitude the sensations of its effect become less and less till they soon entirely vanish, and not the slightest change can be observed in the quality or intensity of the note of the open organ-pipe, whether the resonance box of the fork be open or closed. Indeed at this stage of the experiment the vibrations of the fork may be suddenly and totally stopped without the ear being able to detect the fact. But if instead of stopping the fork when it becomes inaudible we stop the sound of the open organ pipe, it is impossible not to feel surprised at the strong sound of the fork which the open pipe had smothered and had rendered powerless to affect the ear. If we replace the tuning-fork by a closed organ-pipe of the same pitch, the results will be the same; but in this case I adjust the intensity of the higher closed pipe to the point of extinction by regulating the flow of air from the bellows by a valve worked with a screw. The alternation of sound and silence is obtained by closing and opening the mouth of the closed pipe by the hand.

2. *On the Discovery of the Fact that a Sound, even when intense, cannot obliterate the sensation of another Sound lower than it in pitch.*

*High Sounds cannot obliterate Low Sounds.*—A new and remarkable fact was now discovered. No sound, even when very intense, can diminish or obliterate the sensation of a concurrent sound which is lower in pitch. This was proved by experiments similar to the last, but differing in having the

more intense sound higher (instead of lower) in pitch. In this case, when the ear decides that the sound of the (lower and feebler) tuning-fork is just extinguished, it is generally discovered on stopping the higher sound that *the fork*, which should produce the lower sound, *has ceased to vibrate*. This surprising experiment must be made in order to be appreciated. I will only remark that very many similar experiments, ranging through four octaves, have been made, with consonant and dissonant intervals, and that scores of different hearers have confirmed this discovery. It is important to understand that this phenomenon depends solely on *difference* of pitch, and not at all on the absolute pitch of the notes. Thus a feeble  $e'''$  (1024 double vibrations) is heard as distinctly through an intense  $e'''$  (1280 double vibrations) as a feeble  $c$  (128 double vibrations) is heard through an intense  $g$  (192 double vibrations) or an intense  $e'$  (256 double vibrations).

The development of the applications and of the further illustrations of these discoveries would occupy too much space; I must therefore restrict myself to mentioning some of the most interesting. Let a man read a sentence over and over again with the same tone and modulation of voice, and while he is so doing forcibly sound a  $e'$  pipe (256 double vibrations). A remarkable effect is produced, which varies somewhat with the voice experimented on; but the ordinary result is as follows. It appears as though two persons were reading together, one with a grave voice (which is found by the combination of all the reader's real vocal sounds below  $c$  in pitch, or having less than 256 double vibrations), the other with a high-pitched voice, generally squeaky and nasal, and, I need not add, very disagreeable. Of course the aspirates come out with a distressing prominence. I have observed many curious illustrations of this change in the quality of the tone of the voice, caused by the entire or partial obliteration of certain vocal components, while listening to persons talking during the sound of a steam whistle, or in one of our long, resonant American railway carriages. Experiments similar to those on the human voice can be made, with endless modifications, on other composite sounds, as those of reed-pipes, of stringed instruments, of running water, &c. With one of my  $c$  (128 double vibrations) free Grenié reeds, I get very marked results. Using as a concurrent sound an intense  $e'$  (256 double vibrations), I perceive the prime or fundamental simple tone  $c$  to be unaffected in intensity, while all the other partial tones (higher harmonics or overtones, as they are sometimes called) are almost obliterated, except the fifth partial (or fourth upper

partial  $e''$ , of 640 double vibrations, and the sixth partial (or fifth upper partial)  $g''$  (of 768 double vibrations), which come out with wonderful distinctness. The fact that the lowest, or prime partial tone in the majority of ordinary compound musical tones is strongest, is due (among other reasons) to the fact that the sensation of each partial tone of which the whole musical tone is composed, is diminished by the action on the ear of all the components or partial tones *below* it in pitch. Thus the higher the pitch of any component or partial tone the greater the number of lower components which tend to obliterate it. But the prime, or lowest, component partial tone is not affected by any other. Another illustration I cannot resist giving. At the end of the street in New York in which I resided, there is a large fire-alarm bell, the residual sound of which, after its higher components have disappeared, is a deep simple tone. This bass sound holds its own with total indifference to the clatter of horses, or to any sounds *above* it in pitch. It dies out with a smooth gradient, generally without the slightest indentation or break produced by the other sounds of the street. Indeed, in this case, as in all others where one sound remains unaffected by intense higher notes, the observer feels as though he had a special sense for the perception of the graver sound—an organ entirely distinct from that which receives the impress of the higher tones.

That one sonorous sensation cannot interfere with another which is lower in pitch is a remarkable physiological discovery, and, next after the demonstration of the fact that the ear is capable of analyzing compound musical sounds into their constituent or partial simple tones, is probably the most important addition yet made to our knowledge of the nature of hearing. It cannot fail to introduce profound modifications into the hypotheses heretofore framed respecting the mechanism and functions of the ear.

### 3. *On a proposed Change in the usual Method of conducting Orchestral Music, indicated by the above discoveries.*

We have seen how an intense sound may obliterate, entirely or in part, the sensations of certain partial tones or components of any musical tone, and thus produce a profound change in its quality. In a large orchestra I have repeatedly witnessed the entire obliteration of all sounds from violins by the deeper and more intense sounds of the wind instruments, the double-bases alone holding their own. I have also observed the sounds of the clarinets lose their peculiar quality of tone and consequent charm from the same cause. No doubt the conductor of the orchestra heard all his violins,

ranged as they always are close around him, and did not perceive that his clarinets had lost that quality of tone on which *the composer* had relied for producing a special character of expression.

The function of the conductor of an orchestra seems to be threefold. First, to regulate and fix the time. Secondly, to regulate the intensity of the sounds produced by the individual instruments, for the purpose of expression. Thirdly, to give the proper quality of tone or *feeling* to the whole sound of his orchestra, considered as a single instrument, by regulating *the relative intensities* of the sounds produced by the various classes of instruments employed. Now this third function, the regulation of relative intensities, has hitherto been discharged through the judgment of the ears of a conductor who is placed in the most disadvantageous position for judging by his ears. Surely he is not conducting for his own personal gratification, but for the gratification of his audience, whose ears stand in very different relations from his own in respect of their distance from the various instruments in action. Is it not time that he should pay more attention to his third function, and place himself in the position occupied by an average hearer? This position would be elevated, and somewhere in the midst of the audience. The exact determination of its place would depend on various conditions which cannot now be considered. That the position at present occupied by the conductor of an orchestra has often allowed him to deprive his audience of some of the most delicate and touching qualities of orchestral and concerted vocal music I have no doubt, and I firmly believe that when he changes his position in the manner now proposed, the audience will have some of that enjoyment which he has too long kept to himself. During the past winter, in the Academy of Music at New York, and this spring at Offenbach's concerts, I fully confirmed all the foregoing surmises, by placing myself in different parts of the house to observe the different results; and my opinions were fully shared by others who have a more delicate musical organization than I can lay claim to.

In large orchestras these interferences of sonorous sensations are so multiplied and various as to be beyond our mental conception. By taking them up in detail, however, some general laws may be evolved. But it will be impossible to formulate such laws until, first, we are in possession of a *quantitative* analysis of the compound tones of all musical instruments (that is, until we know the relative loudness of the partial tones of which they are composed at all parts of

their compass), and, secondly, we have determined throughout the musical scale the relative intensities of the sounds (of simple tones) when obliteration of the sensations of higher (simple) tones supervenes. The powerlessness of one sound to affect the sensation due to another sound lower than itself in pitch greatly simplifies this problem.

4. *Applications of the Interferences of Sonorous Sensations to Determinations of the Relative Intensities of Sounds.*

Quantitative analysis of the compound tones of musical instruments is now the great desideratum of the composer. It is only after we know the relative intensities of the components of typical musical tones used in orchestral performances that we can so regulate their intensities as to give those qualities of sound which the composer desires to be heard. Thus it at once becomes evident that the instruments used in orchestral music should be very differently constructed from those used for solos or quartets. In orchestral instruments certain *characteristic* upper partials (overtones, harmonics) should predominate in order to find expression in the midst of other and graver sounds. Such orchestral instruments will therefore have exaggerated peculiarities in their qualities of tone which will render them unfit to be played on alone and uninfluenced by other orchestral notes. It is surely not hopeless to anticipate that empirical rules may be attained, which will guide the musical-instrument-maker to the production of those special qualities of tone required in orchestral instruments. It is fortunate that the very phenomena of the interferences of sonorous sensations will assist in the much-desired solution of the problem of measuring the intensity of a sound (simple tone), either when existing alone or as component of an ordinary musical (compound) tone. On this subject I am now engaged. It is evident (by way of illustration) that, so far as concerns the measure of the relative intensities of sounds *of the same pitch*, this problem has already received the simplest solution by merely placing these sounds at various distances and obliterating the sensations they excite by means of a constant and standard sound of a lower pitch. But I reserve a description of this work for a more formal publication.

LXII. *Attempt at a Theory of the (Anomalous) Dispersion of Light in Singly and Doubly Refracting Media.* By Professor E. KETTELER.

[Continued from p. 422.]

12. **I**N the present investigation we now come to perhaps the most essential difficulty which has hitherto stood in the way of the extension of Fresnel's theory of reflection to anisotropic media.

In the universal æther as first medium let a wave-plane polarized under any azimuth be supposed to fall upon a plane face of a crystal with any orientation whatever ; there will in general arise, besides the reflected wave, two refracted ones. In order to determine them according to amplitude and azimuth, we require, as with the transmission of light into an isotropic medium, four different limit-equations. But while (on account of the divisibility of the fundamental theorem of the *vires vivæ*) two principles there suffice for their derivation, three are necessary here. We designate them briefly as the principle of so-called continuity, the principle of equality of work, and the principle of *vires vivæ*. The first and third we owe to Fresnel and Neumann, while the second is, as far as I know, new.

I. *The principle of the equivalence of the quantities of motion or continuity parallel to the dividing surface.*—Let the whole of the vibrations be referred to a system of coordinates whose Z-axis falls in the vertical direction, its Y-axis is perpendicular to the plane of incidence, and its X-axis is consequently the intersection of the planes of incidence and division. Further, let the vibrations  $\rho$  of the æther particles of the boundary layer be analyzed parallel to the axes into the components  $\xi$ ,  $\eta$ ,  $\zeta$ . The fundamental principle of continuity then requires that for the two former parallel to the dividing plane, therefore parallel as well as perpendicular to the plane of incidence, the sum of the components of the velocities of vibration in the incident and reflected wave be equal to the sum of these components in the two refracted waves. We have consequently :—

$$\left. \begin{aligned} \frac{d(\xi_E + \xi_R)}{dt} &= \frac{d(\xi'_D + \xi''_D)_0}{dt}, \\ \frac{d(\eta_E + \eta_R)}{dt} &= \frac{d(\eta'_D + \eta''_D)_0}{dt}, \end{aligned} \right\} z=0. \quad \dots (27)$$

I particularly remark that this fundamental theorem is to be referred only to the complete excursions—that is, to those which have hitherto been denoted by  $\rho_0$ , and their amplitude,



in contradistinction to  $A$ , by  $A_0$ . In reference to this distinction, however, the mere geometrical continuity is, as we see, not sufficient. We have much rather to consider the *vis viva* of the individual æther particles adjacent to the dividing surface as a pushing force  $m \frac{d\rho}{dt}$ ; and now not merely will the quantity of the motion parallel to the plane  $z=0$  in the first medium be equal to that in the second, but it is evident that in an outward direction it can be represented only by the amplitude  $A_0$  of the refracted waves and not by  $A$ . If the continuity alone indeed gave the measure, we could for media in motion, as for those at rest, exchange  $\rho$  and  $\frac{d\rho}{dt}$  for one another at pleasure, which is demonstrably wrong. Corresponding to this requirement, in the foregoing equations the  $\xi_0, \eta_0$  of the refracted waves also are externally indicated.

Since these equations preserve their validity moreover for all points ( $z=0$ ) of the continuously successive wave-planes which fall into the dividing surface itself, it seems appropriate here to refer the excursions not, as above, to the direction of the ray, but to the normal, and therefore to put generally

$$\rho_0 = A_0 \cos \frac{2\pi}{T} \left( t + \frac{z \cos \alpha + x \sin \alpha}{\omega} - \Theta \right),$$

where  $\alpha$  denotes successively the angles of incidence, reflection, and refraction, and by  $\lambda = \omega T$  is to be understood the wavelength along the normal.

I moreover take leave, differing from Cauchy, who treats all velocities of propagation alike, to characterize them according to their direction, by opposite signs. I thus write them more naturally  $\left( \Theta = \frac{D}{\lambda_E} \right)$ :—

$$\left. \begin{aligned} \rho_E &= A_E \cos 2\pi \left\{ \frac{t}{T} - \left( \Theta - \frac{z \cos \alpha_E + x \sin \alpha_E}{\lambda_E} \right) \right\}, \\ \rho_R &= A_R \cos 2\pi \left\{ \frac{t}{T} - \left( \Theta + \frac{z \cos \alpha_R + x \sin \alpha_R}{\lambda_R} \right) \right\}, \\ \rho_D^0 &= A_D^0 \cos 2\pi \left\{ \frac{t}{T} - \left( \Theta + \frac{z \cos \alpha_D + x \sin \alpha_D}{\lambda_D} \right) \right\}. \end{aligned} \right\} (28)$$

If, lastly,  $U, V, W$  are the angles made with the axes of coordinates by the directions  $\rho_0$  perpendicular to the ray, and if we insert these values in the limit-equations, they subdivide, so far as they remain valid for all values of  $t$  and  $x$ , into the

following :—

$$\left. \begin{aligned} \frac{\sin \alpha_E}{\lambda_E} &= -\frac{\sin \alpha_R}{\lambda_R} = -\frac{\sin \alpha'_D}{\lambda'_D} = -\frac{\sin \alpha''_D}{\lambda''_D}, \\ A_E \cos U_E + A_R \cos U_R &= (A'_D \cos U'_D + A''_D \cos U''_D)_0, \\ A_E \cos V_E + A_R \cos V_R &= (A'_D \cos V'_D + A''_D \cos V''_D)_0. \end{aligned} \right\} (29)$$

The first leads, with respect to the angles  $\alpha$ , to the values

$$\alpha_E = e, \quad \alpha_R = 360^\circ - e, \quad \alpha_D = 180^\circ + r,$$

where  $e$  and  $r$  are absolute quantities. In the last two, again, on account of the presupposed state of rest of the medium, the velocity-amplitudes are proportional to the excursion-amplitudes.

II. *The principle of the work perpendicular to the dividing surface.*—If the preceding fundamental principle corresponded to the law of equivalence of pushing force or apparently also to Cauchy's assumption of the continuous reciprocal thrust of each two corresponding curves represented by the resultant oscillation-velocities of the æther particles in the first and second media as functions of their position  $(x, y, z)$ , on the other hand the principle now in question represents an evidently mechanical requisite. There remains, namely, always to bear in mind that the originally interfering spontaneous force which causes the vibratory motion has a certain mechanical work to accomplish, which consists, when the force operates in pure æther, in overcoming its force of elasticity—and when in the interior of a ponderable medium, in overcoming not merely the elasticity of the æther, but also the resistance of the corporeal particles. In each case this work will be comparable to the pulling-open of an elastic spring or the raising of a weight. The mechanical work once applied, maintains itself then, by means of the deformation-force of the æther, in the totality of the vibrating particles as their energy. If therein we fix our attention upon a single particle of æther, on its part it accomplishes now negative work, when it freely follows the pull of the elastic force and transforms its tension into *vis viva*, and now positive work, when the acquired *vis viva* expends itself and, through the increasing resistance which it encounters, is retransformed into tension.

As regards particularly those æther particles which are immediately adjacent to the dividing surface, they are compelled by the  $Z$  component of the incident and the reflected wave now to approach, now to recede from this limit-plane; and a particle which in the position of equilibrium lies in that plane itself is impelled alternately into the upper and the under me-

dium. The oscillations of these particles, however, pass regularly only when they have equal mechanical work to accomplish in a vertical direction on both sides of the plane  $z=0$ .

Now, if a weight  $p$  to be raised corresponds to the resistance in the first medium, and likewise  $p'_D$   $p''_D$  denote the relative resistances of the refracted wave in the second medium, then the principle of equality of work requires

$$p(d\zeta_E + d\zeta_R) = p'_D d\zeta'_D + p''_D d\zeta''_D,$$

or, if, so far as these weights come out proportional to one another, we pass by integration from the time  $dt$  to  $t$ ,

$$p(\zeta_E + \zeta_R) = p'_D \zeta'_D + p''_D \zeta''_D, \quad z=0. \quad . \quad . \quad (30)$$

Here  $\zeta$  self-evidently refers to the actual excursions  $\rho$ ,  $A$  in contrast to the, under circumstances, possible  $\rho_0$ ,  $A_0$ . As regards now the ratio of these resistances, in the first medium the vertical raising of the weight  $p$  to the height 1 produces for the æther-mass  $m$ , which is thereby at the same time displaced in its obliquely situated path-line, an effective *vis viva*  $mc^2$ . Likewise, in consequence of an equal change of level of the weight  $p_D$ , an equal æther-mass  $m$  in the second medium receives the same *vis viva*  $mc^2$ ; but simultaneously the neighbouring corporeal mass  $\Sigma m'$  receives the energy  $\Sigma m'c'^2$ ,—both taken in their respective path-lines. Evidently we have then

$$p : p_D = mc^2 : (mc^2 + \Sigma m'c'^2).$$

But it has already been shown that, when in any refracting medium a given quantity of work is distributed to corporeal and æthereal particles, the ratio of the distribution becomes identical with the so-called refracting force. Corresponding to this, becomes

$$p : p_D = 1 : n^2. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

And thus, if we designate the angle between the direction of vibration and the vertical by  $\mathfrak{A}$  and put, analogously as above,

$$\rho = A \cos \frac{2\pi}{T} \left\{ t + \frac{z \cos \alpha + x \sin \alpha}{\omega} - \Theta \right\},$$

the principle of the equality of the work perpendicular to the dividing surface receives the final form

$$A_E \cos \mathfrak{A}_E + A_R \cos \mathfrak{A}_R = A'_D \cos \mathfrak{A}'_D n'^2 + A''_D \cos \mathfrak{A}''_D n''^2, \quad (32)$$

where self-evidently  $n'$ ,  $n''$  denote here the corresponding sine-ratios.

It will be observed that this fundamental principle, although Fresnel entirely disregarded it, and Cauchy by bringing in longitudinal waves more circumscribed it externally, is an ex-

tremely close approximation to the proper course of the bending or splitting of the incident wave.

The two principles hitherto advanced separately, now find their complement and combination in

III. *The principle of the conservation of the vires vivæ.*—This receives at once, in consequence of the above, first the form

$$\left. \begin{aligned} M \left[ \left( \frac{d\rho_E}{dt} \right)^2 - \left( \frac{d\rho_R}{dt} \right)^2 \right] &= M'_D \left( \frac{d\rho'_D}{dt} \right)^2 + \Sigma \mathfrak{M}'_D \left( \frac{d\mathbf{r}'_D}{dt} \right)^2 \\ &+ M''_D \left( \frac{d\rho''_D}{dt} \right)^2 + \Sigma \mathfrak{M}''_D \left( \frac{d\mathbf{r}''_D}{dt} \right)^2, \end{aligned} \right\} z=0, \quad (33)$$

where the  $M$ 's are the so-called optical equivalents—that is, the spaces passed through by the waves in equal times. And when the previous values of  $\rho$  are introduced and the vibrations of the corporeal particles ( $\mathfrak{M}$ ,  $\mathbf{r}$ ) immediately eliminated by means of equation (26), it is simplified into

$$M(A_E^2 - A_R^2) = M'_D n'^2 A'_D{}^2 + M''_D n''^2 A''_D{}^2. \quad (34)$$

As regards the ascertaining of the volumes  $M$ ,  $M_D$ , let those wave-portions within the medium be considered which are reflected at a dividing surface with finite limits (*e. g.* of angular form, one side of which is parallel to the  $X$ -axis), or enter through it into the second medium. During the time  $T$  these wave-portions are displaced along the direction of the ray to distances proportional to the previous wave-length  $\lambda$ ; and the parallelepipedal spaces described by them, of the height  $l$ , are just those sought. They can next be replaced by their halves—that is, by the so-called Huyghens's prisms, immediately contiguous relatively to the dividing surface, and the volumes of which are evidently as the lengths  $h$  of the perpendiculars let fall from the respective points of contact of the wave-surfaces upon the dividing surfaces. Consequently  $M : M_D = h : h_D$ .

13. This being presupposed, the question is merely to determine the angles  $U$ ,  $V$ , which the perpendiculars let fall, each in its vibration-plane, upon the four rays make with the  $X$ - and  $Y$ -axes, and, further, the angles  $\mathfrak{W}$  between the direction of vibration and the  $Z$ -axis, as well as the heights  $h$  of the Huyghens's prisms.

For this purpose let the axes  $OX$  and  $OZ$  be constructed in the plane of the paper as the  $XZ$  plane (plane of incidence), and  $OY$  perpendicular to them. Let  $OS$  be the direction of the ray, and  $ON$  that of its normal, which in the plane of  $XZ$  is distant the refraction-angle  $r$  ( $=\alpha_D - 180^\circ$ ) from  $OZ$ . If, finally, we draw the line  $OR$  in the plane  $SON$  and perpendi-

cular to OS, the question is, the angles it makes with the axes. If now we join the points X, N, Z, also R, N, S, as well as R, X; R, Y; and N, Y, at the unit of distance from O by arcs of a circle, there will result the two spherical triangles, RNX and RNY.

In the first we have

$$\cos RX = \cos RN \cos XN + \sin RN \sin XN \cos RNX,$$

or, since  $RS = 90^\circ$ ,  $NS = \Delta$ , and the surface-angle  $RNX = \theta$ , i. e. equal to the azimuthal angle of the vibration-plane ROS and the plane of incidence XOZ,

$$\begin{aligned} \cos RX &= \cos (90^\circ - \Delta) \cos (90^\circ - r) \\ &\quad + \sin (90^\circ - \Delta) \sin (90^\circ - r) \cos \theta; \end{aligned}$$

$$\cos U = \sin \Delta \sin r + \cos \Delta \cos r \cos \theta.$$

The triangle RNY has the side  $YN = 90^\circ$ , and the surface-angle YNR, which is distant the azimuth  $\theta = 90^\circ$  from YNX. Hence we find

$$\begin{aligned} \cos V &= \cos RY = \sin RN \cos RNY \\ &= \cos \Delta \sin \theta. \end{aligned}$$

If, lastly, we draw, in the vibration-plane SNR, and perpendicular to ON, the vibration-direction OR, and join it by the arc RZ with Z, in the resulting triangle RZN the side  $RN = 90^\circ$ ; consequently

$$\begin{aligned} \cos \angle R &= \cos RZ = \sin RN \cos RNZ \\ &= - \sin r \cos \theta. \end{aligned}$$

There still remains the measurement of the perpendicular  $h$ . Again, let the plane of the paper be the plane of incidence, OX the line in which it intersects the dividing surface, and OZ perpendicular to the incidence. The wave-normal ON is cut in the point A, and the X-axis in the point D, by the corresponding wave-plane; DA is then its projection on the plane of incidence. In the wave-plane, therefore in general above or below the plane of the paper, lies its point of contact B with the wave-surface, which determines both the direction OS and that of the vibration AB.

Now, instead of letting fall directly from B a perpendicular upon the dividing surface, let us first drop from B upon DA produced the perpendicular BC meeting it in C, and from C the further perpendicular CE upon the axis OX, which it accordingly meets in E. Then CE is also the prolongation of the perpendicular  $h$ . We have now in succession:—

$$CE = CD \sin r; \quad CD = CA + AD; \quad CA = AB \cos CAB$$

$$= AB \cos \theta;$$

$$AB = AO \tan \Delta = \omega \tan \Delta; \quad CA = \omega \tan \Delta \cos \theta;$$

$$AD = \omega \cot r; \quad CD = \omega(\cot r + \tan \Delta \cos \theta).$$

Therefore, finally,

$$h = CE = \omega(\cos r + \tan \Delta \cos \theta \sin r).$$

For the incident and the reflected wave the values are derived from the expressions here developed by simply putting  $\Delta = 0$ , introducing instead of  $r$  the geometric angle of refraction  $\alpha_D - 180^\circ$ , and then replacing this by  $\alpha_E = e$ ,  $\alpha_R = 360^\circ - e$ .

14. The relations thus won bring the limiting equations (29), (32), (34) first into the form

$$\left. \begin{aligned} (A_E \cos \theta_E + A_R \cos \theta_R) \cos e &= \Sigma A_D^0 (\sin \Delta \sin r \\ &\quad + \cos \Delta \cos r \cos \theta_D), \\ A_B \sin \theta_E + A_R \sin \theta_R &= \Sigma A_D^0 \cos \Delta \sin \theta_D, \\ (A_E \cos \theta_E - A_R \cos \theta_R) \sin e &= \Sigma A_D \cos \theta_D \sin r n^2, \\ (A_{E^2} - A_{R^2}) \sin e \cos e &= \Sigma A_{D^2} \sin r \cos r n^2 (1 \\ &\quad + \tan r \tan \Delta \cos \theta_D), \end{aligned} \right\} \quad (35)$$

where, for abbreviation, the summation-symbol is to be referred to both reflected waves. To these add the equation

$$A_D = A_{D^0} \cos \Delta,$$

and we can in general introduce either the amplitude of continuity  $A_D^0$  or the amplitude of work  $A_D$ .

We suppose the latter done and thus everywhere the sine itself substituted for the sine-ratio  $n$ . Our limit-equations then coincide perfectly with those derived by Fr. Neumann from his theory, if we only substitute for the variables  $\theta$ ,  $A_D$ ,  $A_R$  the three new ones

$$(\theta) = \theta - 90^\circ, \quad (A_D) = \frac{A_D}{n}, \quad (A_R) = -A_R.$$

If we now, with MacCullagh, limit ourselves to the so-called uniradial azimuths (that is, to those azimuths of the incident light which permit only a single refracted ray to arrive), the further transformations become complicated. Neumann accomplished them in a laborious calculation, and found that the multiplication of the first and third of these equations, the subtraction of the resulting product from the fourth, and the division of the difference by the second give the same result as if the operations were performed with one uniradial azimuth and then the summation-symbol prefixed to

the right-hand side. Accordingly we obtain forthwith

$$\left. \begin{aligned} A_E \sin \theta_E + A_R \sin \theta_R &= \Sigma A_D \sin \theta_D, \\ (A_E \sin \theta_E - A_R \sin \theta_R) \cos e &= \Sigma A_D \sin \theta_D n \cos r, \\ (A_E \cos \theta_E + A_R \cos \theta_R) \cos e &= \Sigma A_D \cos r (\cos \theta_D \\ &\quad + \tan \Delta \tan r), \\ A_E \cos \theta_E - A_R \cos \theta_R &= \Sigma A_D \cos \theta_D n. \end{aligned} \right\} \quad (36)$$

For isotropic media, therefore for  $\Delta=0$ , the first two of these equations refer to the first principle case ( $\theta_E=90^\circ$ ), the last two to the second ( $\theta_E=0^\circ$ ).

The coefficient of weakening  $\frac{A_R}{A_E}$ , to be derived from equations (36), has been sufficiently tested by experiment. And if, on the one hand, the amplitude ( $A_D A_D^i$ ) of the light issuing from the hinder surface of a plane-parallel plate after double refraction is deduced immediately by means of the general relation

$$A_D A_D^i = A_E^2 - A_R^2,$$

on the other hand Neumann has likewise amply discussed the interesting subject of the internal reflexion of crystals. Our fundamental theorems would of course lead to identical equations, whereas Cauchy's very ambiguous theory of continuity may, it is true, be extended to the occurrences at the fore surface, by suitable auxiliary assumptions, but not to those at the hinder surface of a crystal. But since with this the advantage claimed for it, that it establishes the elliptical polarization of transparent media on a mechanical basis, becomes quite illusory, that polarization being far more perfectly derived directly from the foregoing formulæ, its principles also may now prove to be outwardly specious, but intrinsically untenable and arbitrary.

I may, then, well conclude this investigation with the proposition that the hypothesis of the convibration of the ponderable particles harmonizes the verified formulæ of Neumann with Fresnel's view concerning the position of the polarization-plane.

### *Generalization of the Results obtained. The Fundamental Law of Dispersion.*

15. The two equations into which the differential equation (11A) divides can without difficulty be extended so that they may be applied immediately to absorbent as to transparent media. If, namely, instead of the abscissa  $r$  (above designated by  $x$ ) referred to the direction of the propagation, we introduce the

coordinates  $z, x$  of the plane of incidence, they are also written thus :—

$$m\Lambda \frac{d^2\rho}{dt^2} + \Sigma m' \Lambda' \frac{d^2\rho'}{dt^2} - e\Lambda \left( \frac{d^2\rho}{dz^2} + \frac{d^2\rho}{dx^2} \right) = 0; \quad \text{. . . (I.)}$$

$$\epsilon\Lambda \left( \frac{d^2\rho}{dz^2} + \frac{d^2\rho}{dx^2} \right) - \epsilon' \Lambda' \left( \frac{d^2\rho'}{dz^2} + \frac{d^2\rho'}{dx^2} \right) + \kappa \Lambda' \rho' = 0. \quad \text{. . . (II.)}$$

In order to integrate these equations, we put

$$\left. \begin{aligned} \rho &= A e^{\frac{2\pi}{\lambda} qz} \cos \left\{ \frac{2\pi}{T} \left( t - \Theta' + \frac{zp + x \sin e}{v} \right) \right\}, \\ \rho' &= A' e^{\frac{2\pi}{\lambda} qz} \cos \left\{ \frac{2\pi}{T} \left( t - \Theta' + \frac{zp + x \sin e}{v} \right) \right\} - 2\Delta, \end{aligned} \right\} \quad \text{(III.)}$$

where, for abbreviation,  $\Delta = \chi' - \chi$ ; further,

$$\alpha_E = e, \quad \alpha_D = r, \quad p = v \cos r, \quad \sin e = v \sin r, \quad p^2 + \sin^2 e = v^2.$$

After the carrying-out of this substitution, equation I. becomes

$$m\Lambda^2 \cos \phi + \Sigma m' \Lambda'^2 \cos (\phi - 2\Delta) = m^2 \Lambda^2 [(p^2 - q^2 + \sin^2 e) \cos \phi + 2pq \sin \phi].$$

It subdivides into the two following :—

$$v^2 - q^2 - 1 = \frac{\Sigma m' \Lambda'^2 \cos 2\Delta}{m\Lambda^2}, \quad 2pq = \frac{\Sigma m' \Lambda'^2 \sin 2\Delta}{m\Lambda^2}. \quad \text{(IV.)}$$

Now, inasmuch as the right-hand side of these expressions, as characteristic of the medium referred to, is independent of the angle of incidence, we have, in accordance with equation (12):—

$$p^2 - q^2 + \sin^2 e = v^2 - q^2 = a^2 - b^2, \quad pq = ab. \quad \text{. . . (V.)}$$

Consequently for the *actual* variable ratio of refraction  $\nu$  (*conf. l. c. p. 70*),

$$2\nu^2 = a^2 - b^2 + \sin^2 e + \sqrt{(a^2 - b^2 - \sin^2 e)^2 + 4a^2 b^2}.$$

Analogous treatment of the differential equation II. conducs to the relations

$$\left. \begin{aligned} (a^2 + b^2)^2 \epsilon' \Lambda'^2 \sin 2\Delta &= \kappa' \lambda^2 [ (a^2 - b^2) \Lambda'^2 \sin 2\Delta \\ &\quad - 2ab \Lambda'^2 \cos 2\Delta ], \\ (a^2 + b^2)^2 (\epsilon \Lambda^2 + \epsilon' \Lambda'^2 \cos 2\Delta) &= \kappa' \lambda^2 [ (a^2 - b^2) \Lambda'^2 \cos 2\Delta \\ &\quad + 2ab \Lambda'^2 \sin 2\Delta ]. \end{aligned} \right\} \quad \text{(VI.)}$$

Combining them with equation IV., and omitting the summation-symbol, we now obtain again directly (that is, without the aid of the complex) the earlier equation (14), and there-with  $a$  and  $b$  immediately as functions of  $\lambda$ .



Accordingly equations I., II., III. may well be accepted as the fundamental law of the theory of dispersion.

Further, for anisotropic media we have, correspondingly,

$$m \left( \mathfrak{A}_x \frac{d^2 \xi}{dt^2} + \mathfrak{A}_y \frac{d^2 \eta}{dt^2} + \mathfrak{A}_z \frac{d^2 \zeta}{dt^2} \right) + \Sigma m' \left( \mathfrak{A}'_x \frac{d^2 \xi'}{dt^2} + \dots \right) \\ - e \left\{ \mathfrak{A}_x \left( \frac{d^2 \xi}{dx^2} + \frac{d^2 \xi}{dy^2} + \frac{d^2 \xi}{dz^2} \right) + \mathfrak{A}_y \left( \frac{d^2 \eta}{dx^2} + \dots \right) \right. \\ \left. + \mathfrak{A}_z \left( \frac{d^2 \zeta}{dx^2} + \dots \right) \right\} = 0, \quad \dots \quad \text{(VII.)}$$

and

$$\left\{ \epsilon_x \mathfrak{A}_x \left( \frac{d^2 \xi}{dx^2} + \frac{d^2 \xi}{dy^2} + \frac{d^2 \xi}{dz^2} \right) + \epsilon_y \mathfrak{A}_y \left( \frac{d^2 \eta}{dx^2} + \dots \right) \right. \\ \left. + \epsilon_z \mathfrak{A}_z \left( \frac{d^2 \zeta}{dx^2} + \dots \right) \right\} \\ + \epsilon' \left\{ \mathfrak{A}'_x \left( \frac{d^2 \xi'}{dx^2} + \frac{d^2 \xi'}{dy^2} + \frac{d^2 \xi'}{dz^2} \right) + \mathfrak{A}'_y \left( \frac{d^2 \eta'}{dx^2} + \dots \right) \right. \\ \left. + \mathfrak{A}'_z \left( \frac{d^2 \zeta'}{dx^2} + \dots \right) \right\} \\ + \kappa (\mathfrak{A}'_x \xi' + \mathfrak{A}'_y \eta' + \mathfrak{A}'_z \zeta') = 0, \quad \dots \quad \text{(VIII.)}$$

at least so far as the individual molecular qualities are grouped around similarly situated axes.

Here the first equation refers to both ellipsoids ( $\mathfrak{E}$ ,  $\mathfrak{E}$ ), the second only to Plücker's direct one ( $\mathfrak{E}$ ).

And, lastly, as regards circularly polarizing media, the above differential equations need only, in accordance with a remark of MacCullagh's, a simple additional term in order to take in these media also. If we imagine in them a wave following the Z-axis, we shall have as highly probable

$$\left. \begin{aligned} m \mathfrak{A} \left( \frac{d^2 \xi}{dt^2} + \frac{d^2 \eta}{dt^2} \right) + \Sigma m' \mathfrak{A}' \left( \frac{d^2 \xi'}{dt^2} + \frac{d^2 \eta'}{dt^2} \right) - e \mathfrak{A} \left( \frac{d^2 \xi}{dz^2} + \frac{d^2 \eta}{dz^2} \right) &= 0, \\ \gamma \mathfrak{A} \left( \frac{d\eta}{dz} - \frac{d\xi}{dz} \right) + \epsilon \mathfrak{A} \left( \frac{d^2 \xi}{dz^2} + \frac{d^2 \eta}{dz^2} \right) + \epsilon' \mathfrak{A}' \left( \frac{d^2 \xi'}{dz^2} + \frac{d^2 \eta'}{dz^2} \right) \\ &+ \kappa \mathfrak{A}' (\xi' + \eta') = 0, \end{aligned} \right\} \quad \text{(IX.)}$$

and, in addition, the integral-equations

$$\xi = \mathfrak{A} \cos 2\pi \left( \frac{t}{T} - \frac{z}{l} \right), \quad \eta = \pm \mathfrak{A} \sin 2\pi \left( \frac{t}{T} - \frac{z}{l} \right) \quad \text{(X.)}$$

Here the amplitude-ratio  $\mathfrak{A}' : \mathfrak{A}$ , and with it also the refraction-index  $n$ , appear as independent of the sign of  $\eta$ : we get

$$n^2 - 1 = \Sigma \frac{D \pm Gl}{l^2 - 1},$$

where  $G$  denotes a constant. If we further make the assumption that the medium, though always composite, contains only a single so-called active substance, there results approximately by subtraction

$$(n_1^2 + n_2^2) \left( 1 - \frac{1}{n^2} \sum \frac{DL^2}{l^2 - L^2} \right) = \frac{GL'^2 l}{l^2 - L'^2},$$

inasmuch as, on account of the smallness of the difference of the indices, their sum  $n_1 + n_2$  may be put  $= 2n$ . Consequently, further,

$$\frac{n_1 - n_2}{\lambda} = \frac{1}{l^2} - \frac{1}{l'^2} = \frac{GL'^2}{l^2 - L'^2}. \quad \dots \quad (\text{XI.})$$

But it is known that this expression is the rotation-angle  $\psi$  proportional to the plane of polarization. If the division be effected, since according to experiment  $L'^2$  is everywhere only small, it will be finally written

$$\psi = \frac{R}{l^2} + \frac{S}{l^4}.$$

This relation differs from the dispersion-formula proposed by Boltzmann, and recently verified on quartz by Soret and Sarasin, only by containing  $l$  instead of  $\lambda$ .

#### ADDENDUM.

Since the printing of the preceding memoir for the present year's *Verhandlungen des naturhistorischen Vereins*, I have recognized that the problem treated therein is confined within narrower limits than were necessary, and particularly that the refraction-ratio of very large waves can differ from unity without it being necessary to assume an essential alteration of quality on the part of the internal æther.

While, namely in § 2, for the reasons there alleged, a term  $K\rho$  was regarded as not indispensable, I now add it, and accordingly replace the system of the two equations I. and II. by the following:—

$$mA \frac{d^2 \rho}{dt^2} + \Sigma m' A' \frac{d^2 \rho'}{dt^2} = eA \left( \frac{d^2 \rho}{dz^2} + \frac{d^2 \rho}{dx^2} \right), \quad \dots \quad (\text{I.})$$

$$A \left\{ \epsilon \left( \frac{d^2 \rho}{dz^2} + \frac{d^2 \rho}{dx^2} \right) + \kappa \rho \right\} = A' \left\{ \epsilon' \left( \frac{d^2 \rho'}{dz^2} + \frac{d^2 \rho'}{dx^2} \right) + \kappa' \rho' \right\}. \quad (\text{II. B})$$

These being integrated by means of the expressions III., instead of VI. the two following now join on to the relations IV. and V.:—

$$\left. \begin{aligned} & A^2[\epsilon(a^2-b^2)-\kappa_1\lambda^2] \\ & = A'^2\{\epsilon'(a^2-b^2)-\kappa'_1\lambda^2\}\cos 2\Delta - \epsilon'2ab\sin 2\Delta, \\ & A^2\epsilon 2ab = A'^2\{\epsilon'(a^2-b^2)-\kappa'_1\lambda^2\}\sin^2 \Delta + \epsilon'2ab\cos 2\Delta, \end{aligned} \right\}, \text{(VI.B)}$$

from which are developed

$$\frac{A'^2 \cos 2\Delta}{A^2} = \frac{[\epsilon'(a^2 - b^2) - \kappa_1 \lambda^2][\epsilon'(a^2 - b^2) - \kappa'_1 \lambda^2] + 4a^2 b^2 \epsilon' \epsilon'}{[\epsilon'(a^2 - b^2) - \kappa'_1 \lambda^2]^2 + 4a^2 b^2 \epsilon'^2}$$

$$\frac{A^2 \sin 2\Delta}{A^2} = \frac{2ab(\epsilon_1 \kappa_1 - \epsilon \kappa'_1) \lambda^2}{[\epsilon'(a^2 - b^2) - \kappa'_1 \lambda^2]^2 + 4a^2 b^2 \epsilon'^2}.$$

And if for abbreviation

$$\frac{\epsilon'}{\kappa'} = L^2, \quad \frac{\kappa_1}{\kappa'_1} - \frac{\epsilon}{\epsilon'} = D' \frac{m}{m'} \quad . \quad . \quad . \quad . \quad (A)$$

be put and these values introduced into equation IV., we get, as the law of the curve of dispersions,

$$\left. \begin{aligned} a^2 - b^2 - 1 &= \Sigma \left\{ \frac{m' \epsilon}{m \epsilon'} - \frac{\left( a^2 - b^2 - \frac{\lambda^2}{\bar{L}^2} \right) D' \frac{\lambda^2}{\bar{L}^2}}{\left( a^2 - b^2 - \frac{\lambda^2}{\bar{L}^2} \right)^2 + 4a^2 b^2} \right\}, \\ 2db &= \Sigma \frac{2ab D' \frac{\lambda^2}{\bar{L}^2}}{\left( a^2 - b^2 - \frac{\lambda^2}{\bar{L}^2} \right)^2 + 4a^2 b^2}. \end{aligned} \right\} \dots (B)$$

If only one complex zone is present, so that the summation-symbol may be omitted, (A) and (B) can easily be developed explicitly; and then, if we designate the now occurring limit-values of the refracting force for an indefinitely great and an indefinitely little wave-length as

$$\left. \begin{aligned} n_{\infty}^2 - 1 &= \frac{m' \kappa_1}{m \kappa'_1} \text{ for } \lambda = \infty, \\ n_2^2 - 1 &= \frac{m' \epsilon}{m \epsilon'} \text{ for } \lambda = 0, \\ D' &= n_{\infty}^2 - n_2^2 = D n_{\infty}^2, \end{aligned} \right\} \cdot \cdot \cdot \cdot (C_1)$$

and now write  $\lambda_m = n_\infty L$ , we get :—

$$\left. \begin{aligned} a^2 - b^2 - 1 &= n_\infty^2 - 1 - \frac{n_\infty^2}{2} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right) = \frac{1}{2} \left( n_2^2 + \frac{\lambda^2}{L^2} \right) - 1, \\ 2ab &= n_\infty^2 \sqrt{D - \frac{1}{4} \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right)^2} = \sqrt{n_\infty^2 \frac{\lambda^2}{L^2} - \frac{1}{4} \left( n_2^2 + \frac{\lambda^2}{L^2} \right)}. \end{aligned} \right\} \text{(D)}$$

For  $n_\infty = 1$  they coincide with equations (14B). If from them, in the well known manner, the dispersion-curve also for the real branches be constructed, so that first of all

$$n^2 = \frac{1}{2} \left( n_2^2 + \frac{\lambda^2}{L^2} \right) \pm \sqrt{\frac{1}{4} \left( n_2^2 + \frac{\lambda^2}{L^2} \right)^2 - n_\infty^2 \frac{\lambda^6}{L^2}},$$

is written, then, inasmuch as  $n_2^2$  is put  $= n_\infty^2 (1 - D)$ , the root can be extracted, and we get

$$n = \frac{1}{2} n_\infty \left\{ \sqrt{\left( 1 + \frac{\lambda}{\lambda_m} \right)^2 - D} \pm \sqrt{\left( 1 - \frac{\lambda}{\lambda_m} \right)^2 - D} \right\},$$

which differs from equation (9A) only by this—that here  $n_\infty$  represents an actually existing horizontal asymptote, while there  $n_m$  was taken as the mean ordinate of a continuously curved line. Accordingly the formula in question (and with it also at the same time the curve 9B) would by the present discussion be raised again above the signification of a mere approximative formula. Finally, the hyperbolic curves of the anomalous dispersion are now executed no longer on both sides of the horizontal  $n = 1$ , even for a substance optico-chemically simple, but of the line  $n = n_\infty$ ; and hence the refraction-ratio of all wave-lengths may (and, indeed, not improbably) exceed unity.

For the phase-changes we have the following result. We obtain for the absorption-streaks of a simple substance, according to equations IV., the trigonometric tangent of the phase-difference  $2\Delta$  by division of the two expressions (D): thus we get

$$\tan 2\Delta = \frac{n_\infty^2 \sqrt{4D - \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right)^2}}{2(n_\infty^2 - 1) - n_\infty^2 \left( 1 + D - \frac{\lambda^2}{\lambda_m^2} \right)}, \quad \dots \quad (\text{E})$$

or, for  $D$  small (that is, for narrow absorption-streaks), more simply,

$$\tan 2\Delta = \pm \frac{n_0^2 - n_\infty^2}{n_\infty^2 - 1} \sqrt{\frac{\lambda_0 - \lambda}{\lambda_0 - \lambda_m}} \quad \dots \quad (\text{E}_2)$$

As we now presuppose that  $n_\infty^2$  is always greater than 1,  $2\Delta$  remains less than  $90^\circ$ . The difference of phase rises then from the limit-point  $G'$  on the right (for  $\lambda = \lambda'_0$ ,  $n'_0 > n_\infty$ ) up to a maximal value lying on the middle line itself ( $\lambda = \lambda_m$ ), to sink again to zero in the limit-point  $G''$  (for  $\lambda = \lambda''_0$ ,  $n''_0 < n_\infty$ ) on the left of the same. In correspondence with this, the values belonging to the two real branches, instead of being  $0^\circ$  and  $180^\circ$ , now become the same value  $2\Delta = 0$ .

The amplitude-ratio  $A':A$  becomes, in contradistinction to equation (13), variable within as without the absorption-streak. For the interior of it we find

$$\frac{m'A'^2}{m\Lambda^2} = \sqrt{(n_\infty^2 - 1) \frac{\lambda^2}{L^2} - (n_2^2 - 1)}. \quad (F)$$

It may not, perhaps, appear unsuitable in this place to expand correspondingly the differential equations (1) and (6), and to integrate them by means of the simple expressions (3). They take the form

$$\left. \begin{aligned} m \frac{d^2 \rho}{dt^2} &= e \frac{d^2 \rho}{dx^2} - \alpha_1 \epsilon_1 \frac{d^2 \rho}{dx^2} - \alpha_1 \kappa_1 \rho, \\ &\quad - \alpha_2 \epsilon_2 \frac{d^2 \rho}{dx^2} - \alpha_2 \kappa_2 \rho - \dots, \\ m'_1 \frac{d^2 \rho'_1}{dt^2} &= \alpha_1 \epsilon'_1 \frac{d^2 \rho'_1}{dx^2} + \alpha_1 \kappa'_1 \rho'_1, \\ m'_2 \frac{d^2 \rho'_2}{dt^2} &= \alpha_2 \epsilon'_2 \frac{d^2 \rho'_2}{dx^2} + \alpha_2 \kappa'_2 \rho'_2 \dots \end{aligned} \right\} \quad (G)$$

Integration and the elimination of  $\alpha$  give now

$$\omega^2 \left( m + m'_1 \frac{\kappa_1 l^2 - \epsilon_2}{\epsilon'_1 l^2 - \epsilon'_1} + m'_2 \frac{\kappa_2 l^2 - \epsilon_2}{\kappa'_2 l^2 - \epsilon'_2} + \dots \right) = e;$$

that is,

$$n^2 - 1 = \sum \frac{m'}{m} \frac{\kappa_1 l^2 - \epsilon_1}{\kappa'_1 l^2 - \epsilon'_1}, \quad (H)$$

or also

$$n^2 - 1 = \sum \frac{m' \kappa_1}{m \kappa'_1} + \sum \frac{m' \frac{\kappa_1}{\kappa'_1} - \frac{\epsilon_1}{\epsilon'_1}}{m \frac{\kappa'_1}{\kappa_1} l^2 - l}.$$

Introducing the abbreviations previously used in equation (A), and also putting

$$n_\infty^2 - 1 = \sum \frac{m' \kappa_1}{m \kappa'_1}, \quad n_2^2 - 1 = \sum \frac{m' \epsilon_1}{m \epsilon'_1}, \quad (C)$$

we get again the generalized equation (8), namely

$$n^2 - n_\infty^2 = \sum \frac{D'}{l^2}, \quad (I)$$

and at the same time perceive that all the individual constituents participate in the formation of the two extreme refracting forces.

Naturally the previous assumption  $\frac{\epsilon'_1}{\kappa'_1} = L^2$  (understanding by  $L^2$  a constant independent of the density) has for its consequence the further  $\frac{\epsilon_1}{\kappa_1} = \mathfrak{L}^2 = \text{constant}$ , from which then follows

$$\frac{\epsilon_1}{\epsilon'_1} = c \frac{\kappa_1}{\kappa'_1}, \quad D' = C \frac{m' \kappa_1}{m \kappa'_1},$$

and, at least for substances with a single absorption-streak,

$$D' = C(n_\infty^2 - 1). \quad \dots \quad (\kappa)$$

*The dependence hereby expressed between refraction and dispersion is in fact demonstrated by my experiments on gases.* They show, namely, that in a gas-spectrum the distance between two lines which are sufficiently distant from the middle line, so that in equations (8) and (I) the wave-lengths  $l$  and  $\lambda$  can be exchanged for one another, are proportional to their prismatic deviation.

Further, as regards the treatment of anisotropic media, equation (H) for these should evidently be written thus,

$$n'^2 - 1 = \Sigma \frac{l'^2 - \mathfrak{L}^2}{l'^2 - L^2} \frac{m'}{m} \left\{ \left( \frac{\kappa}{\kappa'} \right)_1 \cos^2 a + \left( \frac{\kappa}{\kappa'} \right)_2 \cos^2 b + \left( \frac{\kappa}{\kappa'} \right)_3 \cos^2 c \right\}.$$

And thence it is obvious, not merely that the reasonings in §§ 7-12 still retain their previous applicability, but also that especially the more rigorous equations VII. and VIII., after corresponding completion, would likewise remain valid.

The same would be the case with respect to equations IX., X., XI. of circularly polarizing media.

The conclusive decision if  $\kappa$  in reality differs from *nil* depends, as it appears to me, on the answer given by experiment to the difficult question whether the refraction-ratio of the metals can or cannot be less than unity.

LXIII. *Further Note in regard to a Multiple Differentiation of a certain Expression.* By J. W. L. GLAISHER, M.A., F.R.S.\*

THE present additional note contains a result of a similar nature to that noticed in the September Number (pp. 208-211), but which follows from the  $(n-1)$ ple integral

$$\int_0^\infty \int_0^\infty \dots \int_0^\infty \exp \left( -x - y \dots - t - \frac{r^n}{xy \dots t} \right) x^{\frac{1}{n}-1} y^{\frac{2}{n}-1} \dots t^{\frac{n-1}{n}-1} dx dy \dots dt = \frac{(2\pi)^{\frac{1}{2}(n-1)}}{\sqrt{n}} e^{-nr}$$

\* Communicated by the Author.

(on which see Liouville's Journal, ser. 2, vol. i. p. 82, 1856). Replacing  $x, y, \dots t$  by  $ax, by, \dots kt$ , and taking  $q = r^n \div (ab \dots k)$ , we find

$$\int_0^\infty \int_0^\infty \dots \int_0^\infty \exp \left( -ax - by \dots - kt - \frac{q}{xy \dots t} \right) x^{\frac{1}{n}-1} y^{\frac{2}{n}-1} \dots t^{\frac{n-1}{n}-1} dx dy \dots dt = \frac{(2\pi)^{\frac{1}{2}(n-1)}}{\sqrt{n}} \frac{\exp \{ -n \sqrt[n]{(ab \dots kq)} \}}{\sqrt[n]{(ab^2 c^3 \dots k^{n-1})}};$$

so that the expression on the right-hand side of this equation is such that, if we denote it by  $u$ ,

$$\frac{d^n u}{da db \dots dk dq} = (-)^n u,$$

giving a theorem which may be enunciated as follows:—

If

$$v = \frac{\exp \{ n \sqrt[n]{(a_1 a_2 \dots a_n)} \}}{\sqrt[n]{(a_2 a_3^2 \dots a_n^{n-1})}},$$

then

$$\frac{d^n v}{da_1 da_2 \dots da_n} = v. \quad \dots \quad (1)$$

This can be readily verified by actual differentiation, if the order of the performance of the differentiations be that of the letters  $a_1, a_2, \dots a_n$ .

If we start with the nple integral

$$\int_0^\infty \int_0^\infty \dots \int_0^\infty \exp \{ -x - y \dots - t - \alpha \sqrt[n]{(xy \dots t)} \} y^{\frac{1}{n}-1} z^{\frac{2}{n}-1} \dots t^{\frac{n-1}{n}-1} dx dy \dots dt = \sqrt[n]{n} \cdot (2\pi)^{\frac{1}{2}(n-1)} \frac{1 \cdot 2 \cdot 3 \dots (n-1)}{(n+\alpha)^n}$$

(Liouville, ser. 2, vol. i. p. 289, 1856), and therein replace  $x, y, \dots t$  by  $ax, by, \dots kt$ , taking  $p = \alpha \sqrt[n]{(ab \dots k)}$ , then we find

$$\begin{aligned} & \int_0^\infty \int_0^\infty \dots \int_0^\infty \exp \{ -ax - by \dots - kt - p \sqrt[n]{(xy \dots t)} \} \\ & \quad \times y^{\frac{1}{n}-1} z^{\frac{2}{n}-1} \dots t^{\frac{n-1}{n}-1} dx dy \dots dt \\ & = \sqrt[n]{n} \cdot (2\pi)^{\frac{1}{2}(n-1)} \frac{1 \cdot 2 \cdot 3 \dots (n-1)}{\{ p + n \sqrt[n]{(ab \dots k)} \}^n} \cdot \frac{1}{\sqrt[n]{(bc^2 \dots k^{n-1})}}; \end{aligned}$$

so that if  $w$  denote the right-hand side of this expression,

$$\frac{d^n w}{dp^n} = \frac{d^n w}{da db \dots dk}. \quad \dots \quad (2)$$

This at once suggests the proper mode of enunciating (1) so as to include this case ; viz. the theorem is:—

If

$$v = \frac{\phi\{x + n\sqrt[n]{(a_1 a_2 \dots a_n)}\}}{\sqrt[n]{(a_2 a_3^2 \dots a_n^{n-1})}},$$

then

$$\frac{d^n v}{dx^n} = \frac{d^n v}{da_1 da_2 \dots da_n};$$

which is evidently true, and gives (1) if the arbitrary sign  $\phi$  be replaced by  $\exp$  ; (2) is of course a very special case.

If the theorem in the September Number be similarly extended, it takes the form:—

If

$$v = \frac{\phi\{x + 2^{2(1-\frac{1}{2^n})} \sqrt[n]{a_n} \cdot \sqrt[4]{a_{n-1}} \cdot \sqrt[8]{a_{n-2}} \dots \sqrt[2^n]{a_1} \cdot \sqrt[2^n]{a_0}\}}{\sqrt[n]{(a_1 a_2 a_3 \dots a_n)}}$$

then

$$\frac{d^{2^n} v}{dx^{2^n}} = \left(\frac{d}{da_0}\right) \left(\frac{d}{da_1}\right) \left(\frac{d}{da_2}\right)^2 \left(\frac{d}{da_3}\right)^4 \dots \left(\frac{d}{da_n}\right)^{2^{n-1}} v.$$

I may here mention two errata in my previous note ; viz. in p. 208, equation (3), and p. 209, line 2, the exponents of  $\frac{d}{da_n}$  should be  $2^{n-1}i$  and  $2^{n-1}$  respectively instead of  $2^n i$  and  $2^n$ .

Cambridge, October 25, 1876.

LXIV. *On a Mechanical Illustration of Thermo-electric Phenomena.* By OLIVER J. LODGE, B.Sc.\*

THE publication of the following speculation requires an apology. It is in continuation of a paper on a Mechanical Illustration of Electric Induction and Conduction, published in the November Number of the 'Philosophical Magazine;' and the sections are numbered on continuously with those in that paper ; but whereas the mechanical analogies suggested in the former part of the paper are useful as mere analogies even if they do not (as I believe they do) throw some light on the actual molecular processes, the present portion contains nothing of sufficient mechanical simplicity to be of much service as mere illustration, and it therefore has little value unless the imaginary mechanical actions are really analogous to those which actually do produce thermo-electric currents. I have accordingly laid

\* Communicated by the Physical Society.



it aside for a couple of months to see if any thing would occur to show the improbability of this being the case; but as any fresh light has appeared rather to confirm than to weaken the hypothesis, I now venture to publish it. But it is necessary to say that, though the hypothesis does, with the help of a few more or less reasonable assumptions, account for all the known phenomena of thermo-electricity, yet it is quite possible that the assumptions made are not the most likely ones, and that the laws deduced are not exactly the true ones. Nevertheless, since they fulfil the necessary conditions as well as the true ones, they are sufficient to show that a consistent account of the facts can be given by means of a hypothesis founded on a connexion between the molecules of matter and electricity which is called "resistance" and which is known to exist, though it is not yet accounted for.

I have preferred to continue speaking in terms of the mechanical model, because discussions concerning molecules and æther are apt to have in appearance the looseness which they very often possess in reality. Moreover one has the advantage of being able to leave the precise relation which the cord is to bear to electricity or to the æther, and which the buttons are to bear to molecules, as well as the real nature of the connexion between the buttons and the cord, unspecified and open to any definition which the vortex theory of matter may suggest.

§ 21. In a previous communication (page 353 of the present volume) the passage of electricity through dielectrics and electrolytes was illustrated by mechanical analogies at some length, but its passage through ordinary metallic conductors (§ 11) was only touched upon sufficiently to enable them to be compared with the other media, and all the details of the process were left unconsidered. In the present paper it is intended to enter into these details; and it is to be regarded as a continuation of the first paper.

The electrical resistance of a metal was represented by the friction to which a cord was subject in passing through a row of nearly smooth buttons fixed to a rigid support (see fig. 3, Plate III.). The cord when moving represented a current of electricity; and the buttons represented the molecules of the conductor. But, since we were not supposing the conductor to be at the absolute zero of temperature, the buttons properly ought not to have been standing still but to have been executing some kind of periodic motion.

Let us then suppose that the rods supporting the buttons are not perfectly rigid, but highly elastic, and that each button is vibrating backwards and forwards in the direction of the length of the cord with a succession of harmonic motions of

short period and very small amplitude. Each button will by its friction tend to move the cord with it. Being all exactly alike, their vibrations will all be isochronous ; but they will be in all manner of phases at any one instant ; so on the average they will neutralize each other's effect on the cord, and the cord will remain stationary without oscillation.

The hypothesis just made, viz. that the molecules of a solid are oscillating, and that the oscillation of any one exerts a certain electromotive force, capable of producing a current except for the circumstance that it is exerted equally in opposite directions in rapid succession, and moreover that it is in general neutralized by the similar but ill-timed action of its neighbours, is the hypothesis whose consequences it is the object of the present paper to trace.

§ 22. First of all, then, there is some energy lost in this motion ; for the buttons are sliding backwards and forwards on the cord, and, though they are tolerably smooth, the friction must ultimately bring them to rest, unless the loss of energy is made up to the body from other portions of the same body or from external sources. The energy which is thus continually being wasted in warming the cord must, I think, be held to correspond to the heat lost by radiation into space ; the loss being compensated, in cases of equilibrium of temperature, by equivalent receipts from neighbouring bodies. But if a set of molecules, or a finite mass of any substance, be isolated from all other bodies and left to itself in free space, the molecules will gradually deliver up their motion to the interpenetrating medium, and will ultimately come to rest. If, then, we calculate the work done in unit time by a button sliding to and fro on the cord, we shall get the rate at which a body cools under these circumstances ; and from this the rate under any other circumstances can be obtained at once by simply subtracting the rate at which it gains heat from the enclosure.

Since we have no data regarding the actual motion of a button, we may as well for the present consider it as simply harmonic ; any other sort of motion will give very nearly the same result. Let the period of the simple harmonic motion of a button be  $2t = \frac{2\pi}{\sqrt{\kappa}}$ , and let its amplitude be  $a$ , so that  $2a$  is the whole length of a button's excursion from right to left. Its velocity at any distance  $x$  from its mean position is  $v = \sqrt{\kappa(a^2 - x^2)}$ , and its maximum velocity is  $V = a\sqrt{\kappa}$ . Now we remember that the retarding force acting between a button and the cord is proportional to  $r$  the specific resistance of the substance or the coefficient of "friction," and to  $v$  the velocity

of the button on the cord at any instant (this is Ohm's law; see § 9 equation (1) and § 10); hence the work done in one excursion from right to left is

$$W = \int_{-a}^{+a} r v dx = \frac{\pi}{2} r a^2 \sqrt{\kappa}.$$

The work done in unit time is therefore

$$\frac{W}{t} = \frac{1}{2} r a^2 \kappa = \frac{1}{2} r V^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and this is the rate \* at which a button loses its energy when left to itself without assistance from others, or the rate of cooling of a substance placed in an enclosure at absolute zero. Hence, assuming that the absolute temperature  $\theta$  is connected with the maximum velocity  $V$  and the atomic weight  $m$  by the relation

$$\frac{1}{2} m V^2 = s\theta,$$

the rate of cooling is given as

$$\dot{\theta} = \frac{s}{J_m} r \theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which would be simply proportional to  $\theta$  (Newton's law), except that the resistance  $r$  itself varies with the temperature.

§ 23. The above conclusion that the radiating power of a body is directly proportional to its specific electrical resistance and inversely as its atomic weight, is not negatived by the experiments of Leslie, and of Provostaye and Desains, as far as they go ; for Leslie's order of the metals in radiating-power agrees nearly with their inverse order in conductivity. Moreover, if

one finds the value of the expression  $\frac{m\theta}{r\theta}$  for the few metals more accurately experimented on by Provostaye and Desains, one gets a number whose constancy is indeed rough but quite as good as could be expected. The only metals apparently whose radiating-powers were ascertained by them were platinum, copper, gold, and silver; and of these gold was only tried as gold-leaf, which is scarcely a fair test, because radiation is more than a surface action. The following Table contains in the first column a mean value of the radiating-power, taken

\* If, instead of assuming a simple harmonic motion, one takes any motion having average velocity  $\frac{2}{\pi}V$ , the rate of doing work will be nearly  $\frac{4}{\pi^2}rV^2$ , which is only slightly different from (1).

from Ganot, art. 364, for the three metals, silver, copper, and platinum (gold is left out because its results are discordant); the second column contains the relative atomic weights; the third column an approximation to the relative electric conductivity of each metal, obtained by taking a mean between the value given by Matthiessen and that given by Becquerel (Miller, vol. i., being my authority); and the last column contains the numbers which ought to be constant.

	$\dot{\theta} : \theta.$	$m.$	$\frac{1}{r}$	$\frac{m\dot{\theta}}{10r\theta}$
Silver .....	2.7	108	100	2916
Copper .....	4.9	63.5	96	2987
Platinum .....	10.1	197	13	2586
Gold .....	4.3	196	71	5985

Taking 2850 as a rough mean value of this constant, one can calculate backwards to the radiating-power for any metal on the same scale. The following Table contains the radiating-power given by the hypothesis for a dozen common metals, most of whose resistances at 0° Centigrade were ascertained by both Matthiessen and Becquerel. The exclamatory mark against gold signifies that its atomic weight has been called 98 because the correct value 196 gives 2.0, which is apparently too small a result.

Metal.	Mean conductivity at 0° C.	Radiating-power at zero Centigrade.
Silver .....	100	2.6
Gold! .....	71	4.1
Copper .....	96	4.7
Cadmium .....	24.1	10.6
Platinum .....	13	11.1
Zinc .....	26.5	16.5
Lead .....	8.3	16.6
Tin .....	13.2	18.3
Iron .....	14.6	34.8
Antimony .....	4.6	50.8
Mercury .....	1.7	83.8
Bismuth .....	1.25	108.6

Concerning this Table, I have to remark that the hypothesis cannot be expected to give correct values for the last three metals; for the electrical resistance was ascertained by experiment on large masses of material, whereas it is the *molecular* resistance which is concerned in radiation; and though

the two are the same in homogeneous material, the experimental value of the resistance is probably too great in the case of crystalline metals like antimony and bismuth, or even perhaps iron. And as regards mercury, its molecular motions cannot be all oscillatory like those assumed in the hypothesis, but must have much of their energy in the form of whatever motion is characteristic of liquidity.

§ 24. Now the researches of Dulong and Petit have indicated the true law of cooling to be

$$\dot{\theta} = p(a^{\theta} - 1), \quad \dots \dots \dots (3)$$

where  $p$  is a constant depending on the nature of the substance, and  $a$  is a number usually stated to apply to all substances and to be equal to 1.0077. As, however, this is only an experimental number, I shall take the liberty of assuming that in general both  $p$  and  $a$  vary with the substance, but that the value of  $a$  is always very close to unity.

Equations (2) and (3) combined give us the law according to which the electrical resistance of a body varies with the absolute temperature, viz.:—

$$r = \frac{Jmp}{s} \cdot \frac{a^{\theta} - 1}{\theta}. \quad \dots \dots \dots (4)$$

I shall denote the constant  $\frac{Jmp}{s}$  by the letter  $h$  for shortness, and shall occasionally use (4) in the expanded form—

$$r = \alpha + \beta\theta + \gamma\theta^2 + \dots, \quad \dots \dots \dots (5)$$

where

$$\alpha = h \log a, \quad \beta = h \frac{(\log a)^2}{2!}, \quad \gamma = h \frac{(\log a)^3}{3!}, \quad \&c.$$

The series is very converging, because  $a$  is so near 1; thus if  $a = 1.0077$ ,  $\log_e a = .0076$ ; and the first two terms will in many cases suffice.

### Digression.

§ 25. In Balfour Stewart's 'Heat,' page 207, it is shown from the theory of exchanges that the internal radiative power of a transparent substance is proportional to the square of its index of refraction, because a small cone of rays issuing normally from some small area inside the body will spread out at the surface into a cone of larger angle, the ratio between the two solid angles being obviously  $\mu^2$ , so that to maintain equilibrium of temperature the internal radiative power must be  $\mu^2$  times that of the surface of the enclosure exposed to the plane surface of the body. There is no ground for denying an index of refraction

tion to metals (cf. Professor Ketteler in the November Number of the 'Philosophical Magazine,' p. 333); so we will suppose the same to hold good with them.

Again, the velocity of light in a medium (*other things being equal*) will be proportional to the square root of the elasticity of the threads joining the buttons (§§ 8 and 18); for the maximum velocity  $V$  of a particle is  $a\sqrt{\kappa}$  (§ 22); and this is related to the velocity of propagation of light  $L$  by the equation

$$V : L = 2\pi a : \lambda,$$

$\lambda$  being of course the wave-length, whence

$$L = \frac{\lambda\sqrt{\kappa}}{2\pi}.$$

But the velocity of light in a medium is known to be inversely as  $\mu$ ; therefore

$$\mu^2 \propto \frac{1}{\kappa} \propto K, \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

(see § 8), which appears to agree with Maxwell's theory that the index of refraction of a substance is nearly equal to the square root of its specific inductive capacity  $K$ .

We have found, then, that the internal radiating-power, which we have shown to be proportional to  $r$ , is also inversely proportional to  $\kappa$ , because directly proportional to  $\mu^2$ ; hence

$$\kappa \propto r^{-1}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

or conductivity for heat and electric conductivity in metals are proportional to one another (for we saw in § 18 that  $\kappa$  was probably proportional to the conductivity for heat); and therefore the conductivity for heat varies with the temperature according to the law

$$\kappa = \frac{g}{h} \cdot \frac{\theta}{a^\theta - 1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is a curve not very different from  $\frac{1}{\theta}$ . It vanishes for

$\theta = \infty$ , and it becomes great and equal to  $\frac{g}{h \log a}$  for  $\theta = 0$ .

The law of the steady conduction of heat parallel to the axis of  $x$  through a slab is

$$\frac{dQ}{dt} + \kappa \frac{d\theta}{dx} = 0.$$

Now Professor Tait, in his 'Recent Advances in Physical Science,' p. 271, says that when every thing has attained a permanent con-

dition, the available energy would have a minimum (and therefore a stable) distribution if the temperature fell along a logarithmic curve, or if  $\frac{d\theta}{dx} \propto \theta$ ; it is therefore to be assumed that this is the

case. But from this it follows, since  $\frac{d\theta}{dt}=0$ , that the *thermo-*  
*metric* conductivity  $\frac{\kappa}{\sigma}$  must vary inversely as the absolute tem-  
perature.  $\kappa$  by itself nearly does, but not quite, (8); so we get  
a law for the variation of specific heat per unit volume with  
temperature, viz.

$$\sigma \propto p\kappa\theta = \frac{H}{m} \cdot \frac{\theta^2}{a^\theta - 1} \quad (9)$$

This is tolerably constant, or the curve runs almost parallel to the axis of  $\theta$ , for ordinary ranges of temperature; it attains a maximum about when  $\theta = \frac{2}{\log a}$ , and it has points of inflexion

about when  $\theta = \frac{2 \pm \sqrt{2}}{\log a}$ , the three temperatures referred to being  $-196^{\circ}4$ ,  $-10^{\circ}8$ ,  $+174^{\circ}8$  Centigrade, if the value of  $a$  were  $1.0077$ ; but a very small change in  $a$  would obviously cause great alteration in these temperatures.

One may write a tolerably close approximation to  $\sigma$  for ordinary values of  $\theta$  thus:

$$\sigma \approx \frac{6H}{m(\log a)^2} \cdot \frac{1}{(3 + \theta \log a)}. \quad (10)$$

I have put  $p$  into (9) with no other justification than to get rid of it from  $\kappa$  (whose  $h$  contains it) so as to leave nothing but the atomic weight  $m$  in (9), except a universal constant  $H$ , this  $H$  being proportional to  $m\sigma$ , the atomic heat.

§ 26. In saying, as we did in § 21, that the cord remains stationary under the combined oscillatory motions of the buttons, we have assumed that the friction of a button is symmetrical as regards right and left; that is, we have assumed that the buttons are not notched so as to be rougher when sliding over the cord one way than when sliding the other way, as an ear of rye-grass would be. If this condition were not satisfied, the simple to-and-fro oscillations of the buttons would confer a progressive motion on the cord. Possibly the condition is not satisfied in some crystalline bodies, like tourmaline: and this would account for their internal polarity; for since the crystal is an insulator, the cord will be displaced by the unequal friction only so far as the dielectric elasticity will permit it, and the result will be a state of strain inside the crystal and a difference of potential between its opposite faces. In time the potentials of

these faces would become equalized by surface leakage or otherwise; but any quickening of the motions of the molecules (rise of temperature) will increase the electric displacement and strain, and will restore a certain amount of difference of potential to the faces. On the other hand, a lowering of the temperature of the crystal will give the elasticity an advantage over the friction; some of the previously displaced cord will return, and the potentials of the faces will change sign.

§ 27. Return now to the consideration of a simple metallic conductor with its buttons all executing isochronous simple harmonic motions on the cord. Apply a force to the cord so as to make it move continuously forward with a velocity  $u$ ; that is, generate a current of strength proportional to  $u$  in the conductor by means of external electromotive force. We will not suppose that  $u$  is great enough at all to interfere with the motion of the buttons; in other words, we will assume that  $u$  is incomparably less than  $V$ ; nevertheless the vibrations of the buttons, though unaltered in space, are no longer symmetrical right and left with respect to the cord: in one direction their relative velocity is the sum, in the other the difference of their respective velocities; hence the motion of the cord is resisted, and work must be done to drive it. If the buttons were stationary, the force opposing the motion of the cord in each button would be  $ru$  by Ohm's law; and this force is unaltered by the motion of the buttons so long as  $u$  is small. For consider the work done by a button in one excursion; properly speaking it would be

$$W = \int_{-(a \pm \frac{1}{2}ut)}^{a \pm \frac{1}{2}ut} (v \pm u) r dx, \quad . . . . (11)$$

the signs to be taken according as the excursion is against or with the cord; but as this expression is unmanageably long, I will be content with the simpler one, true when  $u$  is small,

$$W_1 = \int_{-a}^{+a} (v + u) r dx = \frac{\pi}{2} r a^2 \sqrt{\kappa} + 2aru$$

for the work done by a button in an excursion against the cord, and

$$W_2 = \int_{-a}^{+a} (v - u) r dx = \frac{\pi}{2} r a^2 \sqrt{\kappa} - 2aru$$

for the work done in an excursion with it. The balance of work done against the cord in a double excursion (that is, while the button travels over a distance  $4a$ ) is

$$W_1 - W_2 = 4aru; \quad . . . . (12)$$

hence the average force exerted must be equal to  $ru$ .



The rate of radiation work of a button is

$$\frac{W_1 + W_2}{2t} = \frac{1}{2}ra^2\kappa, \quad . \quad . \quad . \quad (13)$$

the same as that given by equation (1) when no current was passing.

We have found, then, that each button exerts an average force on the cord equal to  $ru$ ; hence, if there are  $n$  buttons in a row on each cord, and  $m$  cords lying side by side, the electromotive force or difference of potential required to drive a current of strength  $mu = C$  through a conductor of sectional area represented by  $m$ , and length represented by  $n$ , is

$$\frac{F}{m} = E = nru = \frac{n}{m}r \cdot mu = RC, \quad . \quad . \quad . \quad (14)$$

which merely shows that Ohm's law, if true for each button, is true for the whole conductor if its temperature is kept constant. The work done by this force in any time  $T$  will be equal to the product of the force into the distance through which it may have moved the cord in that time—that is,  $FuT$ ; hence the rate of “frictional” generation of heat by the current is

$$jmnru^2 = jRC^2, \quad . \quad . \quad . \quad (15)$$

which is Joule's law that the amount of heat generated in a given time is proportional to the resistance of the conductor and to the square of the strength of the current. It is convenient to use  $j$  for the number of units of heat contained in a unit of work, *i. e.* for the reciprocal of  $J$ .

Though I have here taken account of more cords than one, it will not be necessary to do this hereafter, as what is true for any one cord will be true for any number, so we will henceforth consider  $m = 1$ .

§ 28. So far we have considered only homogeneous circuits, in which all the buttons have the same amplitude, the same period, and the same average velocities. In these cases the cord is in equilibrium under the united and balanced action of all the buttons on it, but it is perfectly free and capable of permanent displacement by the slightest force; so that if even one button were made to move forward always faster than it returned, it would exert an electromotive force on the cord and move it forward a little at each oscillation. On the other hand, if any button be made to go forward more *slowly* than it returns, it will exert less force on the cord than is expected of it, and the cord will be moved backwards by the other buttons while the slow one is going forward. Hence if we conceive a cord on which all the buttons but one are oscillating regularly to and fro with the average velocity  $v$ , while this

one is oscillating forwards with velocity  $v+x$  and backwards with velocity  $v-x$ , then the cord will be pushed forward *both* by reason of the too rapid motion and by reason of the too sluggish one; and the electromotive force urging it is

$$(v+x)r-(v-x)r=2xr. \quad . \quad . \quad . \quad (16)$$

If the cord were allowed to move, the work done per second by this force would be  $2xru$ ; and  $j$  times this is therefore the rate at which heat is absorbed at such a point by this force. The heat so absorbed is evolved in other parts of the circuit, because friction opposes the motion of the cord with a force  $nur$  equal to  $2xr$ . If we urge the cord at any rate  $u$  through such a circuit by an external electromotive force  $E$  acting in the same direction as the force of the  $x$ -button, the work done per second is

$$Eu=(nur-2xr)u,$$

or the heat generated is

$$j(nu^2r-2xru), \quad . \quad . \quad . \quad . \quad (17)$$

and the strength of the current is

$$u=\frac{E+2xr}{nr}=\frac{E+\Pi}{R}. \quad . \quad . \quad . \quad (18)$$

Now (17) contains two terms, one depending on the square of the current and being the irreversible frictional generation of heat (15), and the other changing sign with the current and representing the heat absorbed or generated at the  $x$ -button according as  $u$  is positive or negative, remembering that the current has been considered positive when going the way the button tends to drive it.

Something analogous to the unsymmetrical action of this  $x$ -button is what I imagine to go on at a junction either of two different metals at the same temperature, or of two parts of the same metal at different temperatures; and accordingly  $2jxru$  will correspond either to the Peltier effect or to Thomson's *convection* effect; and  $2xr$  is analogous to the coefficient which is called  $\Pi$  in the one case and may be called  $\Theta$  in the other.

§ 29. Let us now proceed to consider what kind of unsymmetrical motions may reasonably be expected to actually occur at a junction either of two different metals at the same temperature, or of two portions of the same metal at different temperatures. In both cases the molecules on the two sides of the junction are vibrating at different rates:—in the first case so that

$$m_a v_a^2 = m_b v_b^2, \quad . \quad . \quad . \quad . \quad (19)$$

$m_a$  and  $m_b$  being the atomic weight of the two metals; and in

the second case so that

$$\theta_2 v_1^2 = \theta_1 v_2^2, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (20)$$

$\theta_1$  and  $\theta_2$  being the absolute temperature on either side of the junction.

Now it is very unlikely that the vibrations of the closely packed molecules of a solid should be any thing so simple as the simple harmonic motions partially assumed in §§ 22 and 27 : in other words, the restoring force cannot be due simply to bending of the connecting rods, but it must also be an effect of the neighbouring buttons by impact or otherwise. Any button in the interior of a mass of metal will be driven backwards and forwards with a velocity depending on the average velocities of the buttons in its neighbourhood as well as on the elasticity of its connecting rods ; and if the mass be homogeneous, its velocities to right and left will be equal. But consider a button on the bounding surface of a mass in vacuum. It is being struck behind continually by its contiguous buttons, but there is nothing striking it in front ; and accordingly its velocity in the excursion outwards will be greater than in its return journey, the return having to be accomplished by the strained rods in opposition to the force of impact. Any cause which either increases the average velocity of the molecules or decreases the elasticity of the rods will increase the difference between the out and in velocities, which may be said to depend on  $v$  directly and on  $\sqrt{\kappa}$  inversely. Now a rise of temperature does both these things ; it increases  $v$  in proportion to  $\sqrt{\theta}$ , and it decreases  $\kappa$  in the proportion of  $\frac{1}{\theta}$  (see (8)) ; hence the difference of velocity

may without violence be assumed to be simply proportional to the absolute temperature in any given medium. As the temperature rises, then, a surface molecule will go forward more and more rapidly and return more and more slowly; moreover its mean position will be shifted more and more outwards: occasionally a molecule may get struck away altogether, which is evaporation.

If we bring a second mass of the same material at the same temperature into close contact with the first mass, the surface buttons of each will drive one another inwards, and they will oscillate like any other buttons in the interior of a homogeneous mass. But if the second mass of metal be either of different material or of different temperature from the first mass, its molecules will not be oscillating at the same rate; and though the surface-buttons of each will be partially restored to their places and made to oscillate more uniformly

than before, yet, since the impacts of one are more violent than those of the other, they will still have some irregularity—those of the hotter metal A going faster from A towards B and slower in the reverse direction, while those of B go slowly from B to A and faster back again from A to B. On the whole, then, we have a *double* layer of buttons oscillating quicker in one direction, viz. A B, than the other\*; and such buttons, if threaded on a cord, would have a tendency to drive it in the direction A B, as we have already seen, § 28.

§ 30. At a junction of two metals, or of unequally hot pieces of one metal, we appear then to have found an electromotive force acting in one direction; but there is something further to be noticed yet. We found in the former paper that in any complete model it was necessary to have not one cord but two, arranged side by side and connected so that however one moves the other must move with the same velocity in the opposite direction—one representing a current of positive, the other the simultaneous equal opposite current of negative electricity. Now the electromotive force whose existence we have been recently imagining, is one that will act on both these cords; and if it acted on both equally they would be both urged equally in the same direction, and consequently could not move at all. It is necessary, therefore, to assume that the bite of the buttons on one of these cords is greater than their bite on the other; but whether it is the positive or the negative cord on which the greatest force is exerted in a given case will depend on the nature of the metal, and possibly also on its temperature—though we have no evidence as regards the latter point, and I shall not assume it.

What we have now assumed amounts to this, that the quantity  $r$ , called the specific resistance of a substance, is made

\* Although we should have at such a junction molecules travelling quicker in one direction than the other, there will be no excess of momentum either way as was to be expected in dielectrics and electrolytes (see § 8 footnote and § 16); for there will be a large number of molecules side by side in such a layer, and, since each takes a longer time over its slow journey than over its quick one, there will be at any instant more molecules moving slowly than quickly, and their excess in number will make up for their defect in velocity, so that the average momentum each way will be the same. But it is a suggestion of Mr. G. Johnstone Stoney's, which he made at the Physical Society last November, in connexion with his theory of Mr. Crookes's radiometer, that though  $mv$  may equal  $m'v'$  it by no means follows that  $\frac{1}{2}mv^2 = \frac{1}{2}m'v'^2$ : there will, in fact, in the above arrangement be an excess of kinetic energy in favour of the quickly moving molecules, *i. e.* from A to B. This is energy which can be expended in driving the cord. But if the cord be forcibly held so that it cannot move, the energy will still, I suppose, be transferred from A to B; but it will not now be doing useful work, it will be degraded into mere diffusion of heat.

up of two parts, such that  $r = r' + r''$ , whereof  $r'$  is the specific resistance referred to positive electricity, and  $r''$  is the specific resistance referred to negative electricity, and that in any metal in which a current exhibits *convective power for heat* the two parts are not equal. If the "specific heat of vitreous electricity" is positive (as Thomson has experimentally proved it to be in copper),  $r''$  is greater than  $r'$ ; but if it is negative (as it is in iron), then  $r'$  is greater than  $r''$ . However, this will appear more fully presently, § 35.

§ 31. Consider now the electrical condition of a homogeneous mass of metal. Its surface molecules are moving more quickly outwards than inwards, and are hence straining the cords outwards (which cord they strain most depends upon circumstances; but for simplicity I will suppose that there is only one, and that the positive cord): the cord inside is thus in a slight state of tension, or (§ 1) the potential of the metal is lower than that of the air in its neighbourhood. The cord does not move, because the surface forces are balanced all round: it resembles a drop of water in a capillary tube. (If the negative cord were acted on most, the condition of the metal would resemble mercury in a capillary tube, or simply a drop of water in its own skin.) Another mass of different material will show the same thing, only to a different amount. If the two metals are put into contact, we have three kinds of bounding surfaces, viz. those which separate air and A, A and B, B and air; and the forces at each surface are in general different: hence the forces all round A are no longer balanced, and the cord moves, say from A to B, a certain limited extent, just as the water in two capillary tubes of different bore would when they are brought into contact. The particles of the dielectric outside (being also threaded on the cord) will at the same time be pulled nearer to A and pushed further from B; that is, §§ 1 and 2, the surface of A is charged negatively and that of B positively.

§ 32. The difference of potential produced by the above surface-action is closely allied to Volta's contact electricity, which is also an effect taking place at the free surfaces of the two metals, but which is probably greatly assisted by an incipient voltaic current taking place through the air which acts as the exciting fluid, the electromotive force being produced by the tendency of the air to unequal chemical action on the two metals. Since the air is a dielectric and not an electrolyte, there is no continuous current developed, but only a difference of potential capable of causing a current if the resistance of the air were diminished sufficiently; and the cord is displaced through the air and across the junction of the two metals to

an extent proportional to this difference of potential, the electromotive force being balanced by the slight state of strain which is set up in the air by this displacement. The metals which show the greatest effect are those which stand furthest apart in the voltaic series.

But the contact electricity spoken of above as generated at the junction of two metals has nothing to do with chemical action. It is an incipient thermo-electric current taking place across the junction, which only shows itself as a slight displacement of the cord, unless the metallic circuit is completed by a second junction generating an electromotive force different from that of the first. The action between zinc and copper would not be at all so strong as between bismuth and antimony : in fact, the metals stand here in their thermo-electric order. Of course, what is actually observed in any ordinary case of two metals in contact is a mixture of the two effects ; and the voltaic effect is usually far the greatest.

§ 33. The very provisional value  $2\pi r$  already obtained in § 28 for the electromotive force at a junction is sufficient to show several things. For instance, since it is a direct function of resistance, substances of high specific resistances may be expected to make the best thermo-electric materials—such, for example, as metallic alloys, or sulphides, or, still better, tellurium or selenium\*. Hence also we might expect contact effects to be extremely powerful between two substances ordinarily called “insulators.” Some violence might have to be used to displace the cord through the very rough buttons of a dielectric ; and a certain amount of heat would be generated by the “friction” of the cord through the buttons ; but when once displaced it would not easily slide back again, and the effects might accumulate ; so that by rubbing a dielectric with a cord-supplying substance (a metal), one could electrify it very highly ; and this is done in an ordinary glass electrical machine. Any two substances which differ at all in conducting power may be expected to give some contact-force. Thus, if a crystalline metal conducts better along the fibre than across, or if a strained metal has a higher resistance than an unstrained, or if the conductivity of magnetized iron depends on the direction of magnetization, then contact-force may be looked for in each case at the junction of the substance in its two different states.

§ 34. It is now time to try what are the actual values which

\* Since the resistance of selenium has been found to fall off under exposure to light, it is probable that its “thermo-electric power” will be found to do so also ; in other words, illuminated selenium will have a place higher up in the thermo-electric series than unilluminated selenium.

the assumptions founded on the model give for the electromotive force developed at junctions, and to see if these values satisfy the conditions which Thomson has laid down for them from thermodynamic principles. Consider, therefore, a junction of two metals A and B, at the temperature  $\theta$ , whose resistances are  $r_a$  and  $r_b$ , such that  $r'_a$  and  $r''_a$  are the positive and negative resistances respectively of the metal A (referring to § 30 for an explanation of these terms), and  $r'_b$ ,  $r''_b$  of B; also let  $v_a$  and  $v'_a$  be the out and in velocities (§ 29) of the molecules of the metal A, and  $v_b$  and  $v'_b$  those of the metal B: then the force exerted on the positive cord is

$$F = (v_a - v'_a)r'_a - (v_b - v'_b)r'_b. \quad . \quad . \quad . \quad (21)$$

Now in § 29 we agreed to consider the difference of the out and in velocities to be proportional to the absolute temperature in any particular metal\*: hence we shall have

$$F = A\theta r'_a - B\theta r'_b;$$

similarly for the force acting on the negative cord we shall have

$$F' = A\theta r''_a - B\theta r''_b.$$

Hence the balance of force useful for urging the cords is

$$\Pi = F - F' = \theta \{A(r'_a - r''_a) - B(r'_b - r''_b)\}. \quad . \quad (22)$$

Now (§ 24)  $r$  is of the form  $h \frac{a^\theta - 1}{\theta}$ ; and from what was said in § 30 it is plain that  $h$  must not only vary for different substances, but must have in general different values for the positive and negative cord in any one substance. Call these values  $h'$  and  $h''$ , so that  $h = h' + h''$ .  $h$  also changes from one substance to another, and so does  $a$ ; but I shall assume that the latter is constant as regards the positive and negative cords; if, however, the sign of the specific heat of electricity in a substance were found to depend upon the temperature, then  $a$  must also be split into two parts  $a'$  and  $a''$ . At present I shall

\* The account given in § 29 of the cause of the difference between the out and in velocities at a junction cannot be held to apply satisfactorily to the case of a junction of two different metals; for it makes the thermo-electric power of a metal to depend on the substance with which it is associated, instead of being a simple property of each individual metal and its temperature—as it is known to be, because the metals can be arranged in a thermo-electric series at any temperature. Hence, whatever influence difference of atomic weight may have in producing the unsymmetrical action very reasonably to be expected at a junction, it cannot act quite in the manner suggested in § 29; for the difference between the out and in velocities of any particular substance must really depend only on its own physical properties, and not on those of the substance with which it is put into contact.

only assume that it is different in A and B, and shall call its value in B,  $b$ . Putting the values of  $r$  into (22), we get

$$\Pi = A(h'_a - h''_a)(a^\theta - 1) - B(h'_b - h''_b)(b^\theta - 1). \quad (23)$$

This is the electromotive force generated at a junction of two metals A and B at the absolute temperature  $\theta$ ; and therefore it is the coefficient\* of the Peltier effect; and the heat absorbed per second at such a junction by a current of strength  $u$  is (§ 28)

$$j\Pi u. \quad (24)$$

The roots of the equation  $\Pi = 0$  will give temperatures at which there is no electromotive force, or at which the two metals are neutral to one another. There are only two positive roots:  $\theta = 0$  is of course one; for then all molecules are standing still. The other will be the neutral temperature of Cumming; a first approximation to it is

$$\theta_0 \doteq \frac{-2A(h'_a - h''_a) \log a + 2B(h'_b - h''_b) \log b}{A(h'_a - h''_a)(\log a)^2 - B(h'_b - h''_b)(\log b)^2}. \quad (25)$$

§ 35. Leave this for the present, and proceed to find the electromotive force generated at a junction of two parts of the same metal A at different temperatures  $\theta_1$  and  $\theta_2$ —or rather the electromotive force generated in a certain length of A, in which the temperature is gradually falling from the value  $\theta_1$  at one end to the value  $\theta_2$  at the other. Consider a small element with temperature  $\theta$ , resistance  $r$ , and out and in velocities of the molecules  $v$ ,  $v + dv$  at one end; and at the other, temperature  $\theta + d\theta$ , resistance  $r + dr$ , and velocities  $v_1$  and  $v_1 + dv_1$ : then the force exerted on the positive cord in this element is, just as in (21),

$$\Delta F = r' dv - (r' + dr') dv_1,$$

or in the limit

$$dF = dv dr', \quad (21')$$

which

$$= A\theta dr',$$

since  $dv$  is proportional to  $\theta$  (§ 29).

Similarly, the force on the negative cord is

$$dF' = A\theta dr'';$$

so the balance of electromotive force in the element is

$$d\Theta = dF - dF' = A\theta(dr' - dr''). \quad (26)$$

\* I find I have not used the symbol in quite its ordinary sense: Thomson calls  $\Pi u$  the heat absorbed, and  $J\Pi$  the electromotive force. I have preferred my notation, however, as the  $J$  occurs more naturally in a quantity of heat than it does in an electromotive force.





Thomson's relation 
$$\frac{d\left(\frac{\Pi}{\theta}\right)}{d\theta} + J \frac{\sigma_a - \sigma_b}{\theta} = 0. \quad [15]^*$$

§ 36. It is easy now to find the total electromotive force of a complete thermo-electric circuit—that is, the electromotive force acting in a ring made of two metals A and B, one of the junctions being at a temperature  $\theta_1$ , the other at a temperature  $\theta_2$ ; for we have simply to add the difference of the two Peltier effects to the difference of the two Thomson effects, (23) and (27), and we get

$$E = \Pi_1 - \Theta_a - \Pi_2 + \Theta_b = A(h'_a - h''_a) \int_{\theta_2}^{\theta_1} \frac{a^\theta - 1}{\theta} d\theta \\ - B(h'_b - h''_b) \int_{\theta_2}^{\theta_1} \frac{b^\theta - 1}{\theta} d\theta, \quad [31]$$

which is evidently equal to

$$\int_{\theta_2}^{\theta_1} \frac{\Pi}{\theta} d\theta, \quad [17]$$

as Thomson has shown it must be.

It is evident also from the form of  $\Pi$ , viz.

$$\Pi_{AB} = P_A - P_B,$$

that 
$$\Pi_{AB} + \Pi_{BC} + \Pi_{CD} + \dots + \Pi_{YZ} + \Pi_{ZA} \equiv 0, \quad [32]$$

provided  $\theta$  is the same for all—or that the electromotive force in a ring formed of any number of metals all at the same temperature is zero. The values found for the effects therefore satisfy all the conditions laid down for them.

§ 37. So far I have written down the results of our hypothesis without any approximation; but simpler and nearly accurate values will be obtained by expanding them all in series and taking the first term or two, or, what comes to the same thing, by using the first two terms only of the expression (5) for  $r$ . Thus, writing

$r'_a \doteq \alpha_0 + \alpha\theta$ ,  $r''_a \doteq \alpha'_0 + \alpha'\theta$ ,  $r'_b \doteq \beta_0 + \beta\theta$ ,  $r''_b \doteq \beta'_0 + \beta'\theta$ , we get at once from the hypothesis all the results in the form given to them by Tait, and experimentally verified by him for moderate ranges of temperature. The contact-force at a junction (22) becomes

$$\Pi \doteq \{A(\alpha_0 - \alpha'_0) - B(\beta_0 - \beta'_0) \\ + \theta[\Lambda(\alpha - \alpha') - B(\beta - \beta')]\}, \quad [33]$$

\* The numbers in square brackets refer to Thomson's equations so numbered in his papers on "The Dynamical Theory of Heat. Part VI.—Thermo-electric Currents," reprinted from the Trans. R. S. Edin., in the Phil. Mag. for March, April, May, and June 1856.

which is of the form

$$X\theta - Y\theta^2;$$

and the neutral temperature of Cumming is

$$\theta_0 \doteq \frac{X}{Y} = -\frac{A(\alpha_0 - \alpha'_0) - B(\beta_0 - \beta'_0)}{A(\alpha - \alpha') - B(\beta - \beta')}. \quad (34)$$

We may write for (33)

$$\Pi \doteq Y\theta(\theta_0 - \theta), \quad (35)$$

which will be precisely Tait's form (see Maxwell, p. 306) provided  $jY\theta$  comes out to be the difference of the specific heats of electricity in the two metals at the temperature  $\theta$ .

To find  $\Theta$ , one has from (26)

$$d\Theta_a \doteq A\theta(\alpha - \alpha')d\theta,$$

or

$$\Theta_a \doteq \frac{1}{2}A(\alpha - \alpha')(\theta_1^2 - \theta_2^2), \quad (36)$$

whence, from the definition of the specific heat (29),

$$\left. \begin{aligned} \sigma_a &\doteq -jA(\alpha - \alpha')\theta, \\ \sigma_b &\doteq -jB(\beta - \beta')\theta; \end{aligned} \right\} \quad (37)$$

and therefore, (33), the condition in (35)

$$jY\theta \doteq \sigma_a - \sigma_b \quad (38)$$

is satisfied.

The total electromotive force in a thermo-electric circuit is obtained, either like (31) by finding  $\Pi_1 - \Pi_2 - \Theta_a + \Theta_b$ , or by forming the integral  $\int_{\theta_2}^{\theta_1} \frac{\Pi}{\theta} d\theta$ ; and its value is

$$E \doteq Y(\theta_1 - \theta_2) \left\{ \theta_0 - \frac{1}{2}(\theta_1 + \theta_2) \right\}. \quad (39)$$

The law was originally given nearly in this shape by Avenarius\*; but he omitted the two Thomson effects, and consequently his formula was erroneous. The above form is that of Tait†, who has verified it for moderate ranges of temperature.

Tait also shows that if  $T_{ab}$  stand for the temperature  $\theta_0$  at which two metals A and B are neutral to one another, and if  $\sigma_a$  be equal to  $k_a\theta$  (where  $k$  is a constant), then must

$$(k_b - k_c)T_{bc} + (k_c - k_a)T_{ca} + (k_a - k_b)T_{ab} \equiv 0.$$

Now from (34) and (37)

$$(k_a - k_b)T_{ab} = A(\alpha_0 - \alpha'_0) - B(\beta_0 - \beta'_0), \quad (40)$$

and the cyclical sum of three, or of any number, of such terms evidently vanishes.

University College, London.

\* Pogg. *Ann.* vol. cxix. (1863).

† Proc. R. S. Edin. 1870-71.

LXV. *Notices respecting New Books.*

*Finite Integrals and the Doctrine of Germs, an Essay* by S. EARN-SHAW, M.A. Cambridge: printed at the University Press, 1876. (Pp. 32.)

THE integration of the equation  $F\left(\frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}, \dots\right)u = 0$ , the general linear equation with constant coefficients, is the subject of the present essay. It is well known that the equation is satisfied by the function

$$u = Ce^{lx+my+nz} \dots,$$

$l, m, n \dots$  being arbitrary constants, provided

$$F(l, m, n \dots) = 0.$$

The author maintains that this is the *general integral*, provided  $l, m, n, \dots$  are *germs*. And a germ is a sort of glorified arbitrary constant, which he defines to be “an essentially indefinite quantity.” But our author’s meaning will come out more clearly if we take a

particular case. Let us then consider the equation  $\frac{d^2u}{dx^2} = \frac{du}{dy}$ .

From what has gone before, it follows that the “germinal” solution of this equation is

$$u = Ce^{mx+m^2y} \dots \dots \dots (1)$$

This may be written in the form

$$u = 1 + \frac{x}{1} \cdot m + \left(\frac{x^2}{1 \cdot 2} + \frac{y}{1}\right)m^2 + \left(\frac{x^3}{1 \cdot 2 \cdot 3} + \frac{xy}{1 \cdot 1}\right)m^3 + \dots$$

The coefficient of each of the powers of  $m$  satisfies the equation and is a *sub-integral*. And the expanded form of  $u$  is the general solution, provided  $m, m^2, m^3 \dots$  be considered as independent constants; i. e. the general integral is

$$u = C + C_1 \cdot \frac{x}{1} + C_2 \left(\frac{x^2}{1 \cdot 2} + \frac{y}{1}\right) + C_3 \left(\frac{x^3}{1 \cdot 2 \cdot 3} + \frac{xy}{1 \cdot 1}\right) + \dots$$

With this understanding as to the meaning of  $m$ , (1) represents the general integral in a finite form.

Germs, according to our author, are of two kinds:—minor germs, which are simple constants added to the variables; major germs, which enter the function in any other way. Sometimes these germs are latent. Thus the minor germs are latent in (1); for if  $x+g$  and  $y+h$  were written for  $x$  and  $y$ , it would merely introduce two factors which would coalesce with the arbitrary constant. It is, however, possible to put the general integral into another form, in which the major germs shall become latent and only the minor germs appear explicitly. This form in the case of the above equation is

$$u = C(y+h)^{-\frac{1}{2}} e^{-\frac{(x+g)^2}{4(y+h)}}, \dots \dots \dots (2)$$

the expressions (1) and (2) being indifferently the general integrals required.

In some cases, as might be expected, an expression containing a germ may be replaced by an arbitrary function of the variables:

thus, if  $W$  is such a function and  $m$  a germ,  $e^{mW}$  is equivalent to  $\phi(W)$ . *E. g.* the equation

$$\frac{du}{dy} = a \frac{du}{dx} + bu$$

has for its general integral  $u = Ce^{lx+my}$ , where  $l$  and  $m$  are germs connected by the equation  $m = al + b$ ; so that the general integral becomes

$$u = Ce^{by} \cdot e^{h(x+ay)},$$

which is equivalent to

$$u = e^{by} \phi(x + ay),$$

the solution in its usual form. This brief notice will, perhaps, be sufficient to direct attention to an essay whose contents are at least ingenious and original, and possibly important.

## LXVI. *Proceedings of Learned Societies.*

### GEOLOGICAL SOCIETY.

[Continued from p. 475.]

November 8th, 1876.—Prof. P. Martin Duncan, M.B., F.R.S.,  
President, in the Chair.

THE following communications were read:—

1. “A short notice of a new exposure of Rhætics near Nottingham, in a letter from E. Wilson, Esq., F.G.S., dated November 3rd, 1876,” in which he said:—

“Since March 1875, when I drew public attention to the occurrence of Rhætics near Stanton, on the new Midland line from Nottingham to Melton (notice whereof has been already communicated to the Geological Society), that section has not been any further exposed. Meanwhile the London and North-Western Railway Company are rendering geological science a service by showing a similar but more complete succession of Rhætics in a cutting close to Barnston, on their new Bingham branch. Here the uppermost Keuper marls, with their gypsum bands, are succeeded as usual by the hard light blue marls (? lower Rhætic), 12 feet exposed. Above these come the ‘*Avicula-contorta*’ shales, consisting below of about 13 feet of ‘paper shales’ with their variable yellow sandstones in their upper portion, and a 2-inch bone-bed replete with the usual fish remains &c. 1 ft. 6 in. from their base, and graduating up into a series of more earthy and thickly laminated dark blue shales, estimated thickness 18 to 19 feet. Odd limestone nodules occur about the middle of this last series; and at from 2 to 3 feet from the top similar nodules set in, becoming rather more plentiful upwards, and in the highest 6 inches forming an incomplete layer. These Rhætics are capped by alternating thin-bedded blue limestones and brown clays (Lower Lias), 10 feet exposed; the lowest limestone of which, at the base, a compact or concretionary bed from 3 to 7 inches thick, contains *Modiola minima* and *Myacites unionides*. These beds appear to be the equivalents of the ‘Firestones’ and ‘Guineas’ of Warwickshire, and of similar beds lately noticed by Mr. Harrison

near Leicester, and to indicate the absence of the 'White Lias' at this point. The dip of all the above beds is the same, viz.  $2^{\circ}$  to  $3^{\circ}$  a little north of N.E. (the direction of the line); and there is no evidence of unconformity. The succession is apparently the same at Barnston as on the Nottingham and Grantham line at Elton,  $3\frac{1}{2}$  miles north-east, between which places and beyond to Orston the *Avicula-contorta* shales form a low but very clearly defined escarpment along their outcrop.<sup>5</sup>

2. "Note on the Red Crag." By W. Whitaker, Esq., B.A., F.G.S.

The object of this paper is to show that what had been taken to be an irregular line of erosion between a certain unfossiliferous sand and the shelly Red Crag (in Suffolk) was really a line of dissolution of shells, the sand being simply Red Crag deprived of its fossils through percolation of water. This is shown to be the case from the facts that in some places lines of bedding and false-bedding extend from the Crag into the sand, and that impressions of Crag shells are found in ironstone in the sand.

3. "On the Kessingland Cliff section, and the relation of the Forest-bed to the Chillesford Clay, with some remarks on the so-called terrestrial surface at the base of the Norwich Crag." By F. W. Harmer, Esq., F.G.S.

This paper was a reply to one read before the Society during last session by Mr. J. Gunn, F.G.S., who affirmed that the Forest-beds in the section were overlain by the Norwich Crag and Chillesford Clay. The author, on the contrary, gave, in the first part of the paper, detailed drawings of the cliff, in which he represented the fossiliferous deposits known as the Kessingland Beds as resting in a gorge cut out of, and consequently newer than, the Chillesford Clay. The beds present in the section, according to his view, are (in ascending order):—

- |                       |   |  |
|-----------------------|---|--|
| The Kessingland Beds. | { | 1. The Chillesford Clay.   |
|                       |   | 2. An unstratified blue clay, penetrated by rootlets, shown occasionally to be underlain by sand and gravel, which, with the clay, contains mammalian remains and freshwater shells. |
|                       |   | 3. A lenticular-shaped bed of laminated clay and sand, with wood débris at its base.   |
|                       |   | 4. The Middle Glacial sand and gravel.   |
|                       |   | 5. The Upper Glacial or chalky Boulder-clay.   |

In the 2nd part of the paper the author urged that the stone bed at the base of the Norwich Crag, which contains marine shells and mammalian remains, was not an old land surface, as asserted by Mr. Gunn and others, but merely the basement-bed of the deposit which overlies it, the mammalian fossils it contains, which are principally worn and fragmentary portions of teeth, being derivative from some older Tertiary deposit, just as are the flints with which they are associated from the Chalk.

4. "Observations on the Geology of East Anglia, &c." By S. V. Wood, jun., Esq., F.G.S., and F. W. Harmer, Esq., F.G.S., &c.

The subjects discussed in this paper were threefold, viz.:—

1. The unfossiliferous sands of the Red Crag.
2. The unconformity between the Lower and Middle Glacial deposits.
3. The mode in which the Upper and Middle Glacial were accumulated.

The views of the authors under the first head were similar to and confirmatory of those advanced in the previous paper by Mr. Whitaker; but they pointed out that the Red Crag, which these sands, in an altered form, represent, could not belong to the Chillesford division of that formation, by reason of the casts of shells which had been preserved not comprising any of the more characteristic Chillesford species, and of their including among them forms confined to the older portions of the Red Crag. They also pointed out that the Chillesford Clay had been removed over all the area occupied by these sands by denudation prior to the deposition of the Middle Glacial, which rests upon these sands wherever they occur. The removal of the Chillesford Clay, the authors consider, was due in part, if not entirely, to the great denudation between the Lower and Middle Glacial, which gave rise to the unconformity discussed under the second head.

This unconformity they illustrate by lines of section traversing most of the river-valleys of Central and East Norfolk and Suffolk. These show that such valleys were excavated after the deposit of the Contorted Drift, and out of that formation and the beds underlying it. They also show that the Middle and Upper Glacial have been bedded into these valleys, as well as spread (the middle only partially, but the upper more uniformly) over the high grounds formed of Contorted Drift out of which they were excavated, and thus generally conceal that deposit, which manifests itself only in the form of occasional protrusions through these later formations, but which they consider constitutes, though thus concealed, the main mass of the two counties.

The authors also describe a glacial bed as occurring at various localities in the bottom of some of these valleys, and which in one case they have traced under the Middle Glacial. This they regard as having been formed in the interval between the denudation of the valleys and their subsequent submergence beneath the Middle Glacial sea; and inasmuch as such valley-bed invariably rests on the Chalk in a highly glaciated condition, they attribute its formation more probably than otherwise to the action of glaciers occupying the valleys during an interglacial interval of dry land. They also suggest that, if this was so, it is probable that the forest and mammaliferous bed of Kessingland, instead of being coëval with the pre-glacial one of the Cromer coast, may belong to this interglacial interval—that is to say, to the earliest part of it, before the glaciers accumulated in the valleys, and when the climate was more temperate, any similar deposits in these interglacial valleys having been for the most part subsequently ploughed out by the action of the glaciers.

In discussing the subject under the third head, the authors point out

the many perplexing features which are connected with the position and distribution of the Middle Glacial formation; and while they admit that as to one or two of these the theory which they offer affords no explanation, they suggest that the theory of this formation's origin which best meets the case is as follows, viz. :—As the country became resubmerged, and as the valley-glaciers retreated before the advancing sea, the land-ice of the mountain districts of North Britain accumulated and descended into the low grounds, so that by the time East Anglia had become resubmerged to the extent of between 300 and 400 feet, one branch of this ice had reached the borders of the counties of Norfolk, Suffolk, Essex, Herts, and Bedford, ploughing out and destroying any Lower Glacial beds that had been deposited over the intervening counties upon which it rested, and over which we ought otherwise, having regard to the depth of the earlier submergence under which they were accumulated, to find them, but do not. The Middle Glacial formation, consisting of sand and gravel, they attribute principally to the action of currents washing out and distributing the morainic material, which was extruded on the sea-bottom by this land-ice—that ice itself, by keeping out the sea over all the country on which it rested (which was then below the sea-level), preventing the deposit of the Middle Glacial in those parts. The termination of this current-action was accompanied by increased submergence, and by a gradual retreat of the land-ice northwards to the mountain districts, until Britain was left in the condition of a snow-capped archipelago, from which eventually the snow disappeared and the land emerged. To the moraine extruded from the base of this ice and into deep water they refer the origin of the Upper Glacial Clay, the moraine material remaining partly in the position in which the ice left it, and partly lifted by the bergs which became detached from the ice. Such part of it as was lifted was dropped over the sea-bottom at no great distance from its point of extrusion; and in that way the marine shells occurring in a seam of sand in the midst of this clay at Dimlington and Bridlington on the Yorkshire coast became imbedded, the mollusca which had established themselves on the surface of this moraine material having been thus smothered under a lifted mass of the same, which was dropped from a berg. The authors point out that precisely in the same way in which the Middle Glacial is found stretching out southwards and eastwards beyond the Upper Glacial Clay in Suffolk and in Herts, and is succeeded by such clay both vertically and horizontally, so does the earlier-formed part of the Upper Glacial Clay, or that with chalk *débris*, stretch southwards beyond the later-formed part, or that destitute of such *débris*, and is succeeded by it, both vertically and horizontally. This, they consider, shows that the Middle and Upper Glacial deposits, which constitute an unbroken succession, were due to the gradually receding position of the land-ice during their accumulation, the sequence being terminated with the Moel-Tryfaen and Macclesfield gravels, which were accumulated during the disconnexion and gradual disappearance of the ice, and while the land still continued deeply submerged.



LXVII. *Intelligence and Miscellaneous Articles.*

ON A NEW DYNAMOMAGNETIC PHENOMENON. BY MM. TRÈVE  
AND DURASSIER.

LET a horse-shoe magnet of any length be covered on one face with varnish, or, better, with a strip of glass. If a cylinder of soft iron be placed upon its neutral part, the cylinder will begin to move toward the poles, which it reaches in a time which is of course a function of the weight of the cylinder and the coercive force of the magnet.

The magnetic attraction, therefore, is here exerted, not in the limited field which has been recognized for it, but *over the whole extent* of the magnet.

Hence results a new mode of valuing magnetic force, *by the mechanical work* which it has performed. The product of the weight of the iron moved, multiplied by the space passed through, divided by the time elapsed, will be the precise measure of this magnetic force. If, for example, the force be determined for three large and three small magnets, identical in shape and weight, and containing respectively 0.25, 0.5, and 1 per cent. of carbon, it will perhaps be possible to define the unit of magnetic force ("the *magnet*"), and fix its equivalence in kilogrammetres.

We had made great efforts to determine the magnetic conductivity of steels in proportion to their content of carbon; but the want of a rigorous mode of valuation of magnetic forces had always prevented us. The phenomenon above described will fill the gap and permit us to accomplish this important purpose. We must here, however, indicate the course we have followed.

If, for instance, a steel with 1 per cent. carbon be taken, of our first series of Creusot, it is known that its coercive force is approximately 47 degrees on the compass.

Let us place it in an induction-coil, like an ordinary electro-magnet, giving to the coil the proportions of length, section, wire, &c. fixed by the formulæ of M. du Moncel for the purpose of obtaining a maximum of magnetic power, and send through it a strong current in the wished-for direction. Under these conditions the magnet gives no longer 47, but 64 degrees. If 47 and 64 were absolute numbers, one could say that the magnet  $A_1$  has 47 of permanent magnetism, but can take 64 in the temporary state. 64 would represent the *magnetic capacity* of the magnet with 1 per cent. carbon—that is to say, the *maximum of magnetism* which it is capable of receiving. The difference between 64 and 47 would give the magnetic conductivity.

Now let the extreme steel of the series be  $E_1$  with 0.25 per cent. of carbon. Its coercive force is 13. If to it be applied the same coil as to  $A_1$ , *since they are identical in form and weight*, its magnetic capacity is found to be 69. The difference between 69 and 13 would be the magnetic conductivity of the steel with 0.25 per cent. of carbon.

We have operated in the same manner for B<sub>1</sub>, C<sub>1</sub>, D<sub>1</sub>, and have drawn up the following Table:—

	Coercive Force.	Magnetic Capacity.
A <sub>1</sub> .....	47	64
B <sub>1</sub> .....	45	66
C <sub>1</sub> .....	42·5	67
D <sub>1</sub> .....	33·5	68
E <sub>1</sub> .....	13	69

The magnetic capacity of soft iron of the same form and weight was 71.

These *approximate* relations show how important it was to seek out a method of rigorous measurement of magnetic power.—*Comptes Rendus de l'Académie des Sciences*, Nov. 9, 1876, tome lxxxiii. pp. 857, 858.

ON SOME REMARKABLE PHENOMENA IN GEISSLER TUBES. BY  
 PROF. E. REITLINGER AND A. VON URBANITZKY.

Having, at the commencement of July last, made preparation for continuing our experiments with the aid of a Geissler mercury air-pump, we filled so-called Wüllner tubes with different gases, and first investigated afresh the conversion of attraction into repulsion by a higher degree of rarefaction. The experiments were made with air, nitrogen, oxygen, hydrogen, and carbonic acid. They established that, with all these gases, between 4 and 12 millims. pressure the electrical luminous column is attracted, but at about 2–1 millim. it is repelled. Between the two phenomena there is an intermediate stage, in which neither attraction nor repulsion is perceptible. For the first occurrence, however, of perceptible repulsion it was the less possible to find with the different gases one and the same degree of rarefaction, as the repulsion and attraction proved to be dependent on the intensity of the current, and, for example, a luminous column which showed itself indifferent to the finger was caused distinctly to recede from the finger by the insertion of a resistance. Now since the gases in the tube themselves offer different degrees of resistance, we had to take into account at the same time the intensity of the current, in order to render the data comparable. The latter has been found to exert the greatest influence; yet up to the present time we have only numerous results to communicate at the proper time respecting that influence, but have ascertained no general law for it: this we shall endeavour to discover. If rarefaction be continued below 1 millim., in all the cases which we have observed, layers are produced as the rarefaction proceeds, of little breadth and constant in position so far as our observation extends, which either do not retreat before the finger at all, or only when the induction-current is very much weakened by the insertion of resistances. In consequence of this circumstance the repulsion exhibits a certain maximum during the rarefaction, which again diminishes when the rarefaction is carried still further. A portion of the experiments were made with a somewhat wider tube without any capillary part, which thus was in a certain sense intermediate between the Geissler

tubes and the so-called electrical egg. Already, when using also the double-barrelled valve air-pump, we had experimented with an electrical egg and atmospheric air and seen a distinct recession of the visible luminous swell in the centre of the egg as the hand was brought nearer from a distance. With five or more centims. distance between the hand and the glass wall of the egg the retreat was very evident; but on the glass being touched, it was changed into an attraction, at least in appearance. When we experimented with the Geissler air-pump and the wide tube above-mentioned, there appeared first, when the induction-current passed through in the usual manner, altogether similar phenomena as in Wüllner's tubes; in particular here also with greater rarefaction the attraction changed into repulsion. But the most striking repulsions were obtained when only one pole of the Ruhmkorff apparatus was connected with one of the wire electrodes of the tube, and the other was led away to earth. In this case a luminous appearance was perceptible, which, of course, shone brighter only with a higher degree of rarefaction; but it then exhibited the repulsion-phenomenon especially distinct and with a distance of several centims. between the hand and the glass tube. Its appearance was essentially different, according as the positive or the negative pole of the induced open current was connected with the wire; and the strength of the repulsion was also in the latter case much more considerable than in the former. This repulsion increased as the rarefaction was pushed further, and was most intense at the highest degree of rarefaction attained, about 0.2 millim. The singularly favourable result of this experiment induced us to put the wire electrode of the tube into conducting connexion with the conductor of a two-disk Winter electrical machine; and in this case also, with very strong rarefaction, the positive luminous tuft was seen to recede. Experiments for the purpose of testing the principal inferences suggested by the phenomena with the unclosed current are already in preparation. This may also be the place to mention that in none of the cases investigated by us did an ebonite plate when brought near attract or repel.

The striking similarity which is shown between the luminous phenomenon in the unclosed current with great rarefaction and the figures of comets, and the great distance at which in this case the repulsion of the approaching conductor yet acts, reminded us afresh of the question (touched upon in our first communication) whether, in the repulsion under consideration, we have not to do with that very force which makes the tails of comets recede from the sun. That the degrees of rarefaction at which the best results are here obtained lie near 0.2 millim. is only favourable to such an assumption. Further, in those Geissler tubes in the collection of the Second Physical Lehrkanzel in which the recession of the electrified luminous gas-columns before the approaching conductor was first observed, the light which appeared, when examined with the spectroscope, showed distinctly upon a less-bright background those three spectral bands which the spectroscope exhibited also in the flame of illuminating-gas, and which are usually designated

as a carbon-spectrum, but which, according to Vogel, have likewise formed the spectrum of comets. Moreover, if it be remembered that the repulsion in question, in itself, requires from the repelling body only the conducting quality, and not any free positive or negative electricity, the expectation will not be considered unwarranted that these experiments will be of importance for the electrical theory of the tails of comets and their repulsion—a theory to which several distinguished astronomers have already turned their attention.

In several tubes even at a pressure of 20 millims. (at which the observations commenced) distinct strata appeared; and while the rarefaction was then continued the connexion between the number of the strata and the rarefaction could be proved. It came out that, so long as the chemical quality of the luminous gas-columns and all the other circumstances except the density could be taken as unchanged, *the number of the strata diminishes in the ratio of the rarefaction*, or, in other terms, *that the interval from the centre of one bright stratum to that of the next increases proportionately to the rarefaction*. This law may be expected to hold good either strictly or at least very approximately; for it appeared correct so far as our observation extended. The intensity of the current, too, has a great influence upon the number and clearness of the strata; indeed, by altering it stratified can be changed into unstratified light, and the converse. By further investigations we shall probably obtain data enabling us here also to state a simple normal relation. The conjecture expressed in our first communication on the phenomena in question, that the magnet increases the number of the strata by condensation, is now verified by the observed increase of the strata proportionate to the density.

It was mentioned in our second communication, that when the tube was filled with oxygen the so-called dark space appeared in grey-green light. This nebulous grey-green light we have obtained, by the employment of the Geissler air-pump after filling with oxygen, in the entire Wüllner tube with the sole exception of the capillary part, and found it accompanied by a very vivid brilliance. The observations made on this occasion would agree better with Sarasin's than with Morren's explanation of brightly luminous Geissler tubes. We must reserve the details for a subsequent full communication, as well as those of the experiments of which the chief results have now been described, and also the numerous more or less important collateral phenomena observed, such as—that the grey-green light in oxygen-tubes is neither attracted nor repelled, that on the rarefaction being increased a bright spiral, wound round the positive wire electrode, is developed, that at the tip of this electrode a very bright starlike point makes its appearance on filling the tube with a carbon compound, that the yellow-green light in the glass, which we mentioned in the first communication, constantly accompanies the higher degrees of rarefaction, that in very many cases the so-called dark space is absent, that the current, long continued, has an analyzing effect, &c.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, Oct. 12, 1876.

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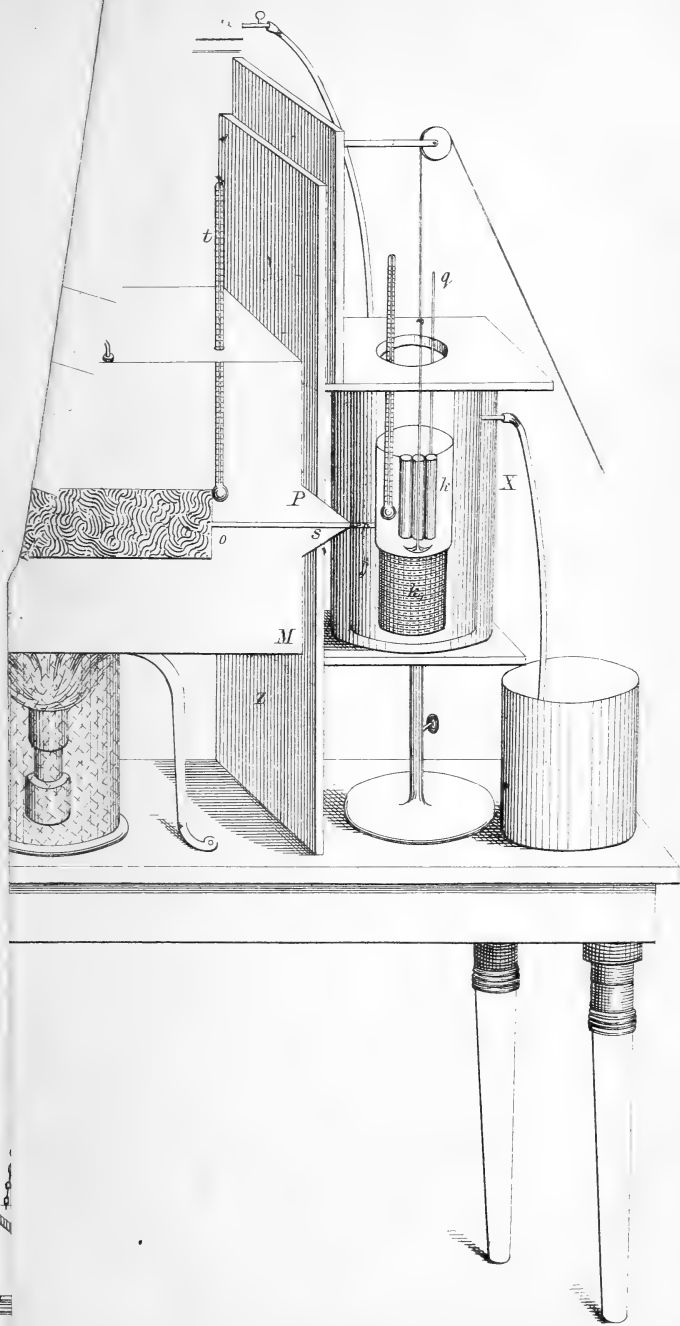
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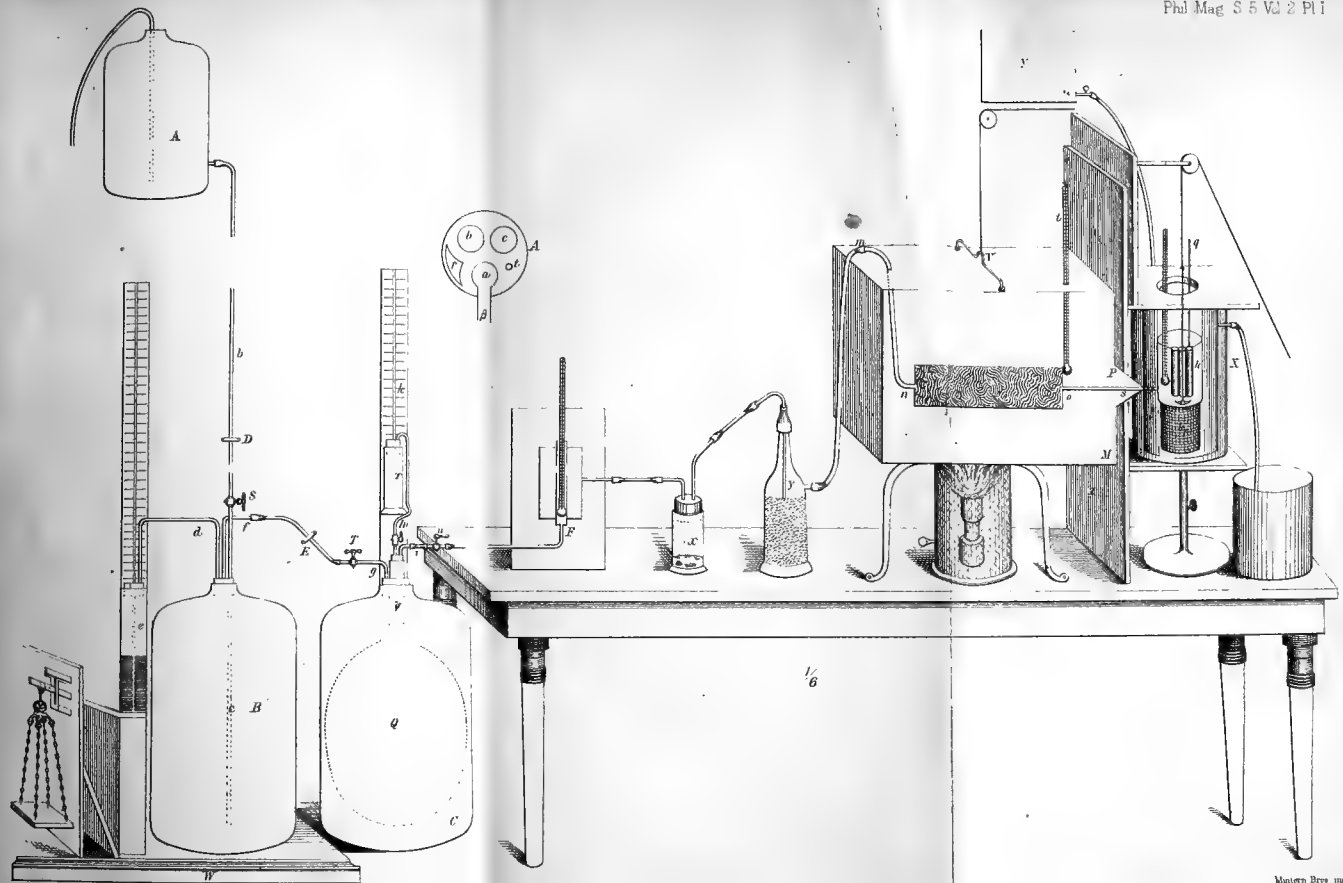
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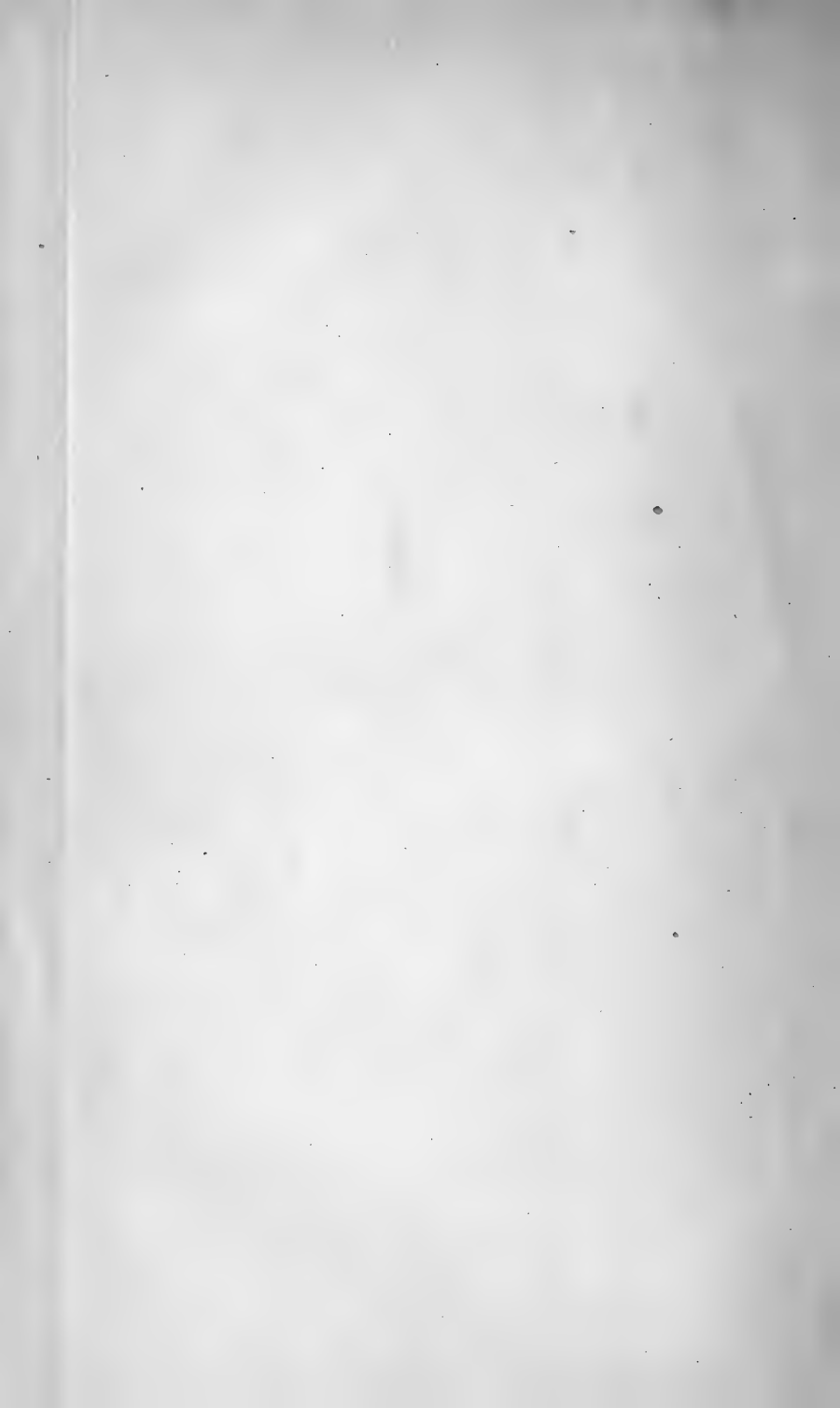


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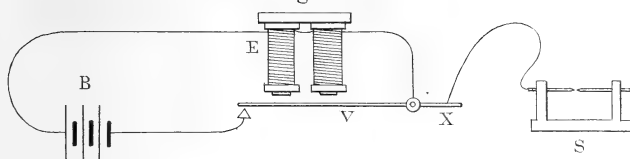


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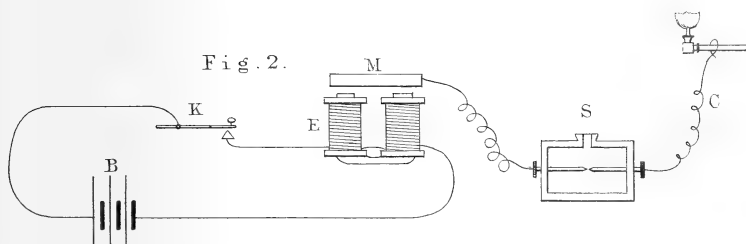


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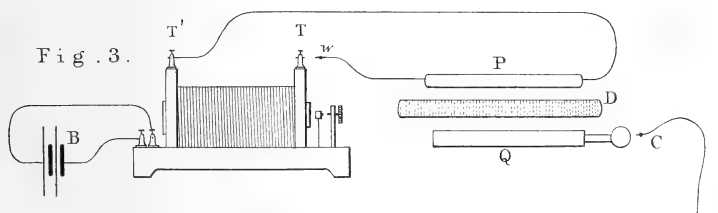


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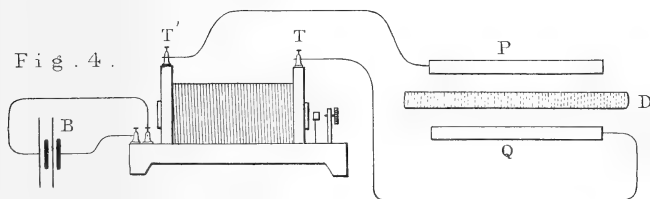


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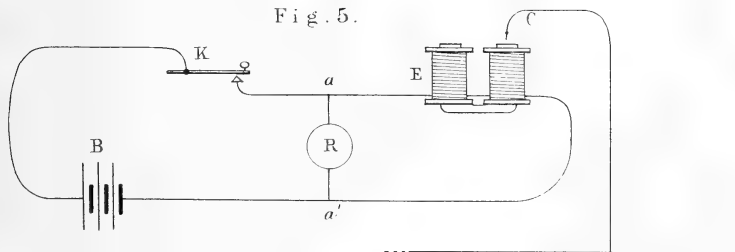




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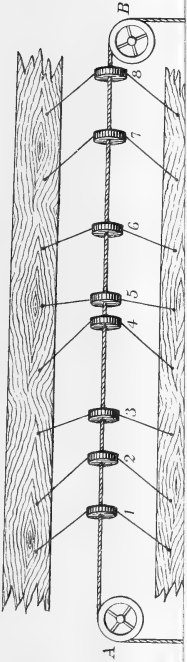


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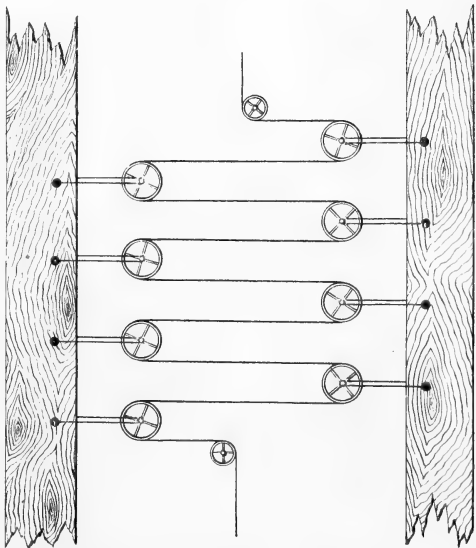


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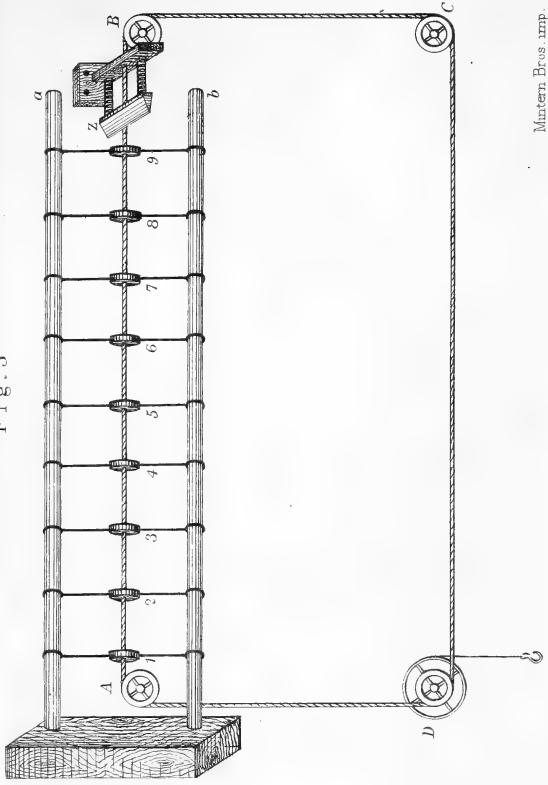






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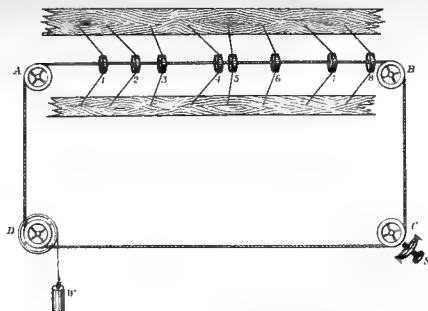


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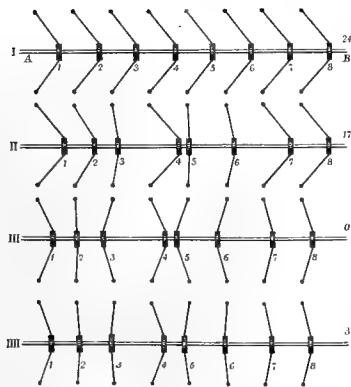


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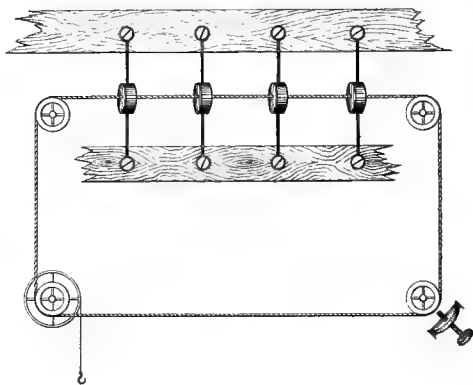


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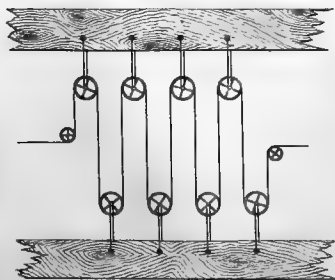


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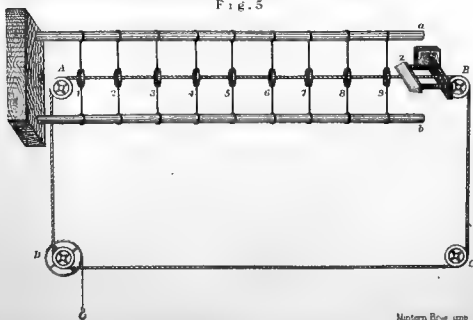




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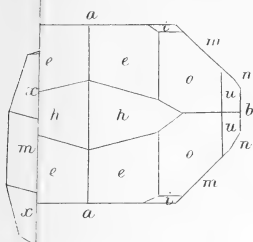
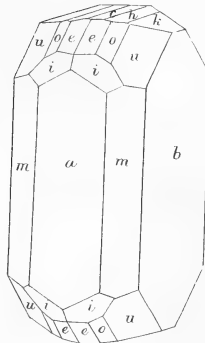


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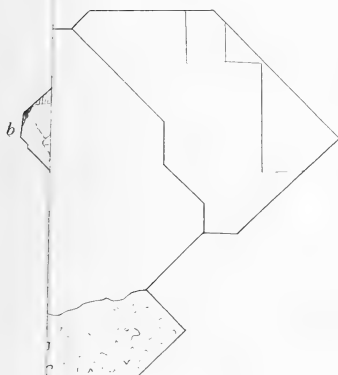


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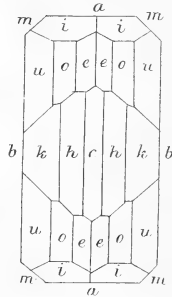


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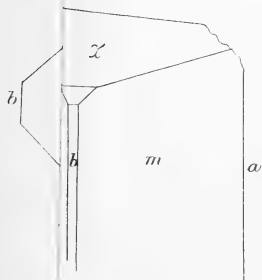


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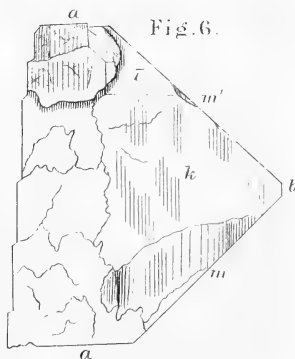




Fig 1



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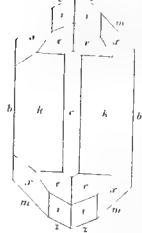


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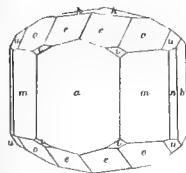


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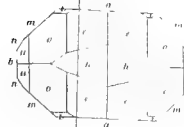


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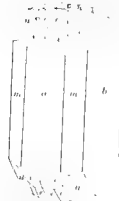


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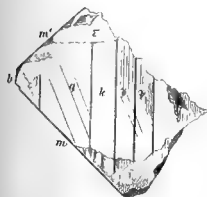


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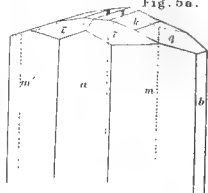


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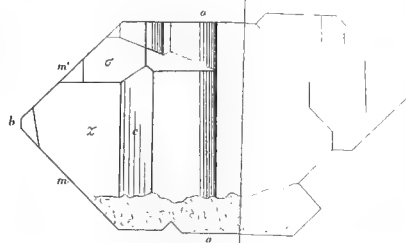


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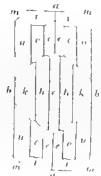


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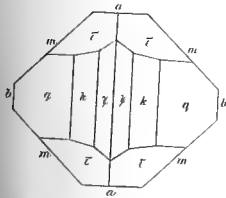


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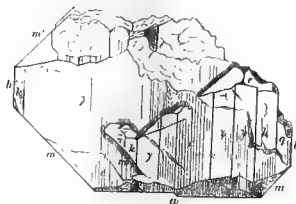


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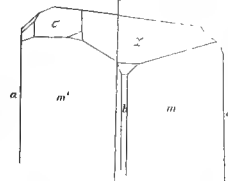


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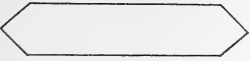


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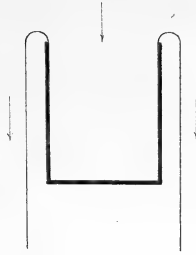


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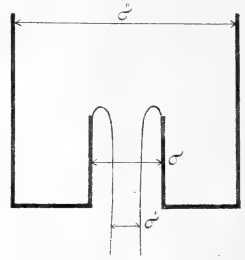


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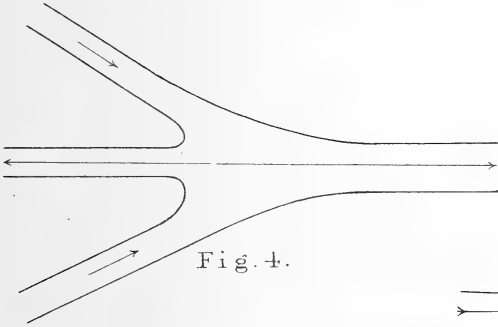


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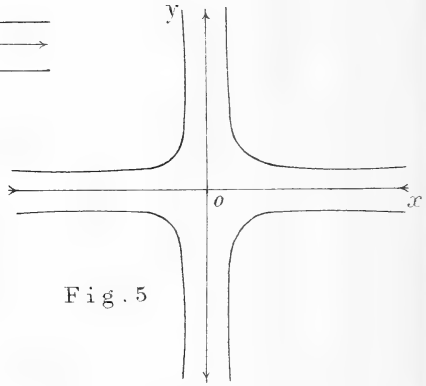


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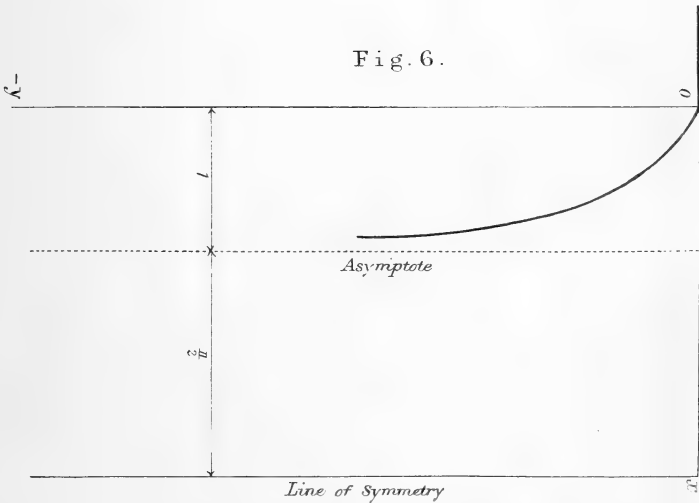


Fig. 6.





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## ERRATA.

Vol. 1, at the bottom of p. 547, for 160 read 160

$$\begin{array}{r} 156 \\ 0 \\ \hline 49 \\ 1 \end{array} \quad \begin{array}{r} 156 \\ 40 \\ \hline 39 \\ 1 \end{array}$$

— page 549, line 5, for 3.  $\overline{U}_{10}^{19}$  read 3.  $\overline{U}_{10}^{19}$

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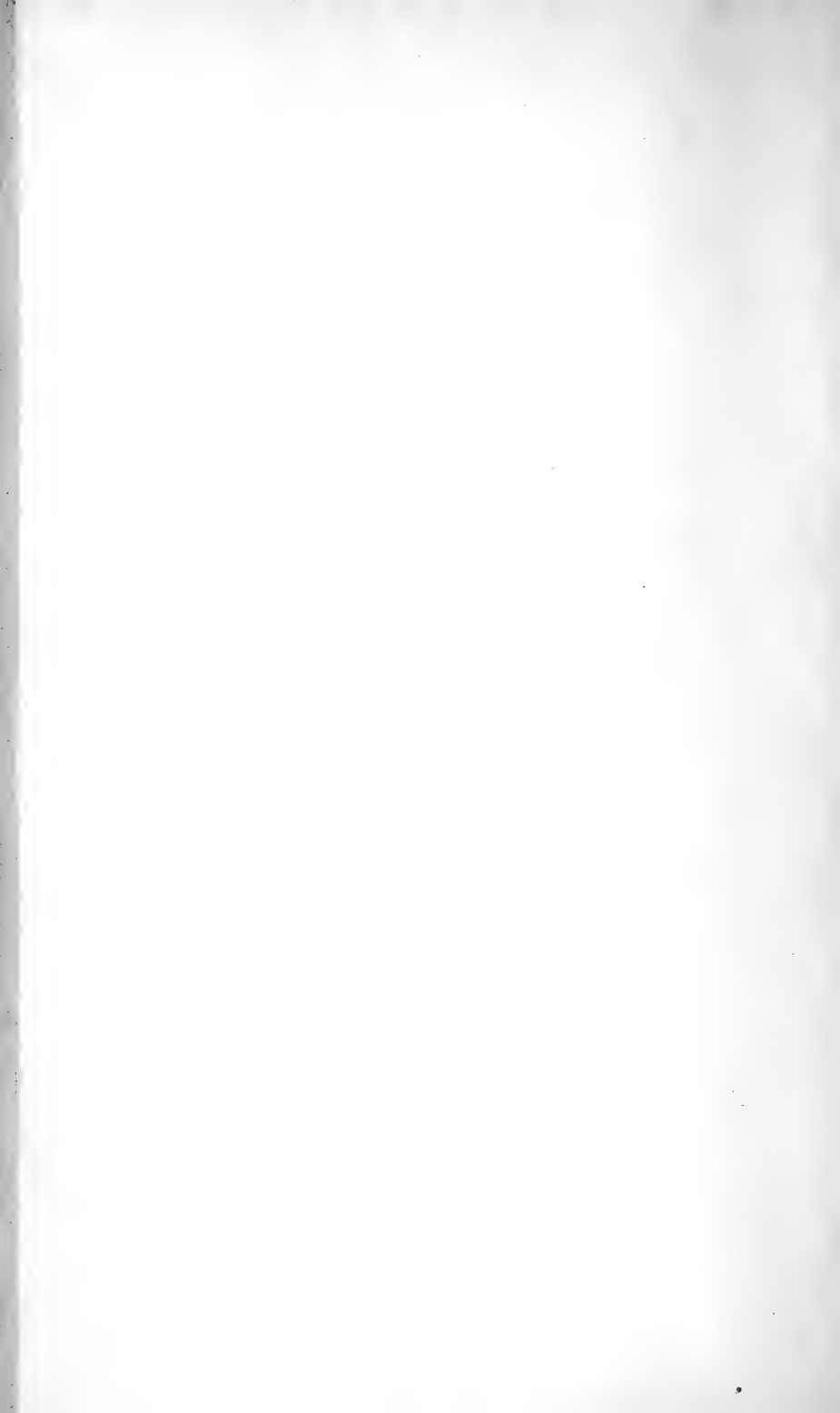
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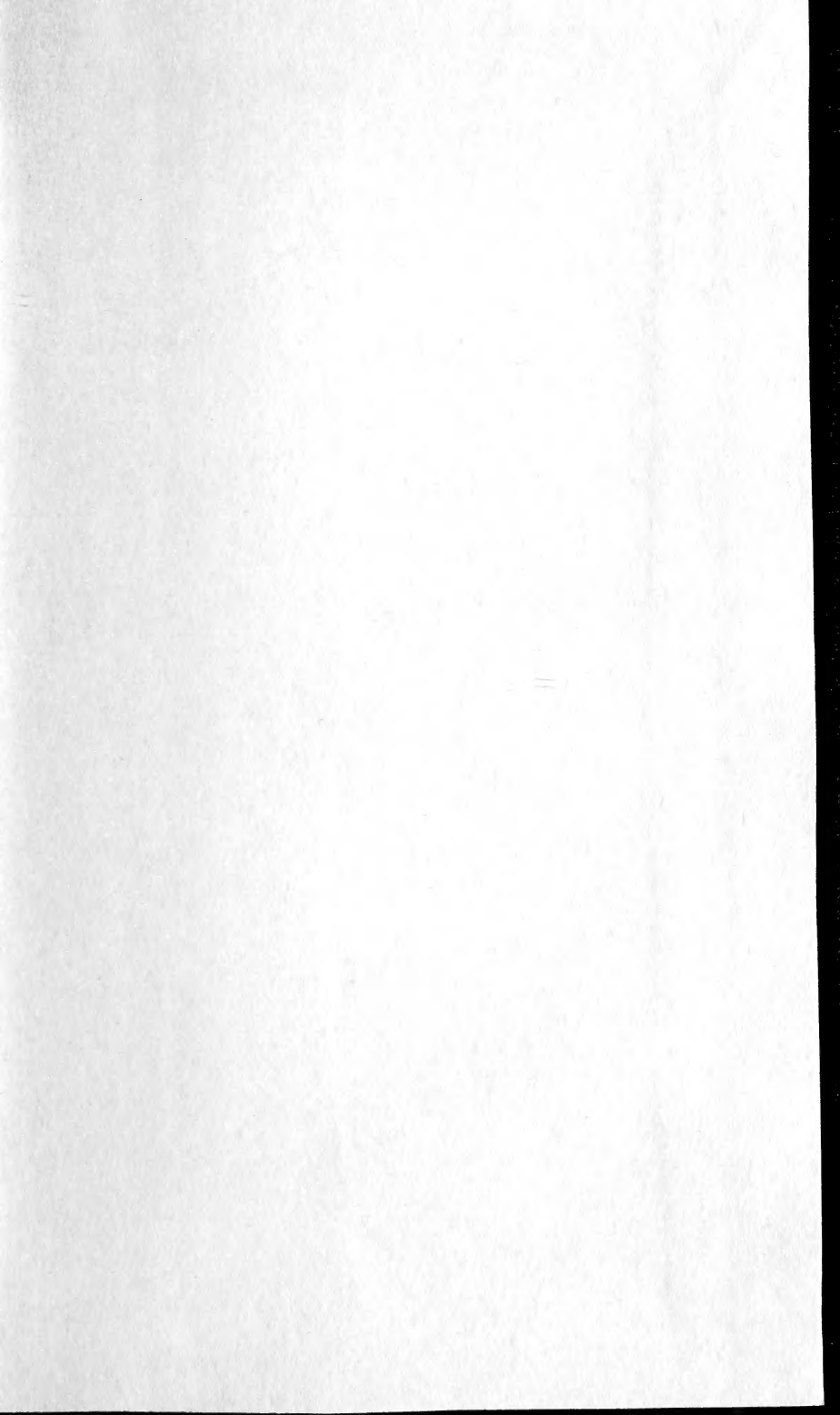
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